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EDINBURGH MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

VOLUME ELEVENTH.

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THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. JANUARY 1815.

PART I.

ORIGINAL COMMUNICATIONS.

I.

On the Operation of tying the Subclavian Artery. By A. Colles, M. D. Professor of Anatomy and Surgery, Royal College of Surgeons in Ireland, and one of the Surgeons of Dr Stevens's Hospital, Dublin.

Soon after I had been raised to the office I now hold in the College of Surgeons, I turned my attention to the study of the subclavian artery. My first object was to discover the most effectual means of compressing the artery, and to ascertain the most eligible spot for this purpose; my next, to investigate the practicability of tying this artery, either before it reaches, or after it has passed through the scaleni muscles. The anatomy of the parts satisfied me of the feasibility of the operation; and Mr John Bell's luminous statement, and lively description of the anastomising arteries situated about the hip, encouraged me to hope for its final success. Still, however, I did not feel myself justified in stating my opinion to the pupils of the college, for, as yet, the operation had not only been untried in practice, but the proposal wanted even the sanction of analogy. At length Mr Abernethy gave to the world the first account of his operation of tying the external iliac artery. From this moment I felt myself authorized openly to express my sentiments; and every year since I have taken occasion, both in the anatomical and surgical course, not only to state to the class my sentiments generally on this subject, but to point out to them particularly the manner in which I conceived the operation might be performed.

A case of axillary aneurism having, in the year ——, come under my care in the hospital, I proposed this operation, on which I had so long meditated, to the other surgeons in consultation. The proposal, however, was overruled, chiefly on the grounds of the operation never having been performed; and I confess that I yielded to the voice of the majority with the less reluctance, because I felt a secret apprehension of some terrible revolution being produced in the frame by tying an artery of such magnitude so close to the heart. This patient, in a few months, fell a victim to the disease; but neither by argument, nor influence, nor stratagem, could I obtain an opportunity of examining the body. Mr Ramsden now published his account of the operation which he performed on this artery, after its passage through the scaleni. Although the result was finally unsuccessful, the experiment served at least to prove to me one point incontestably, namely, that the fear of immediate danger from a general revulsion in the system was totally groundless. Emboldened by the establishment of this fact, I again proposed the operation in the very next case that occurred, and it was unanimously agreed to in consultation. In this case, I undertook to tie the artery before it had yet reached the scaleni. Several months elapsed before another case presented itself; but the patient, in this instance, positively refused to submit to the operation, unless I would assure him of complete success. The unshaken fortitude, and patient resignation with which he submitted to the wide spreading ravages of this painful disease, through all its successive stages, proved that, in this determination, he was not influenced by a desire to avoid the comparatively trifling pain of an operation. Of this case I have related the appearances on dissection, as I think they suggest some useful practical inferences.

The third case was, of all, the most favourable for operation, and yet the result proved unfortunate. In this the artery was tied after it had passed through the scaleni.

Case I.—Michael Cowel, a labourer, aged 33 years, was admitted into Dr Stevens's hospital on 23d September 1811, for an aneurism of the right subclavian artery. He gave the following account of his disease:

Thinks that he repeatedly hurt himself by endeavouring to push on with his shoulder a loaded car, the wheels of which had sunk deep in passing over heavy ground. Soon after these exertions, (perhaps in the course of a week) he began to complain at and near to the clavicle. At this time, too, he felt a tumour the size of a cherry, which throbbed with a continual pulsation. He points out a spot above the middle of the clavicle, and rather towards its humeral extremity, as having been the original seat of the disease. He did, in this early stage of the disease, observe a tumour or pulsation in a spot nearer to the sternum, but says that, as the tumour increased in size, it extended itself in that direction. It was not until the beginning of July he first felt a pain and numbness along the right arm and fore-arm.

At present, the tumour extends from the sternal origin of the sterno-mastoid muscle along the clavicle, until it reaches a little beyond the arch of that bone. It rises in height nearly two inches above the clavicle, and is of a conical form, the apex of the cone being situated at the outer edge of the sterno-mastoid muscle. The finger can be passed with facility between the clavicle and tumour, but at no part can it be passed between the posterior surface of the tumour and the subjacent parts. The aneurismal pulsation is less strong, and the tumour more compressible between the heads of the sterno-mastoid muscle, than at any other part. Pulse 74, and of nearly equal strength in both wrists. Of late he complains of slight numbness in the fingers of his right hand. He speaks of some uneasiness in the chest, which he terms " a draw on his chest." This has been much relieved since yesterday by blood-letting at the arm to the amount of fourteen ounces. His health in every other respect is good.

On Thursday, October 10th, the operation was performed, the patient being laid on his back upon a table, so placed that the direct light fell on the right side of his neck.

Two incisions were made through the integuments; the first running along the middle of the sterno-mastoid muscle, from the highest point of the aneurism to the top of the thorax; the second beginning at the middle height of the first, and ending at the sternal articulation of the clavicle. The integuments being then separated from the subjacent parts, the lower triangular flap of skin was laid down on the thorax, and the upper portion turned upon the side of the throat. The mastoid muscle being thus exposed to view, its different portions were next divided. The sternal portion was first cut through about an inch and a half above its origin. This was done with a sharp-pointed bistoury, passed on a director which had been introduced on the outer edge of the muscle. The clavicular portion was then divided to the breadth of about half an inch, the director being passed on its inner edge. In both these cases, the introduction of the director was much facilitated by slightly scraping along the edge of the muscle. The lower divisions of the muscle were then dissected from the subjacent parts, and turned down over the clavicle. Now the line of the carotid artery could be distinguished, and its pulsations felt by the finger. The sterno-thyroid and hyoid muscles lying exposed, were divided in the same manner, and at the same height from the clavicle, as the sterno-mastoid had been. The division was carried only through so much of their breadth as enabled us fully to expose the carotid artery. We then proceeded to detach the lower divisions of these muscles from the surrounding parts; but here some delay was occasioned by a large vein descending from the anterior part of the throat into the internal jugular. The vein, however, being secured by two ligatures applied at a small distance from each other, and then divided in the interval between them, the inferior portions of the sterno-thyroid and hyoid muscles were raised from their beds, and turned down on the thorax. Now the sheath of the carotid artery was opened by pinching up a small portion of it with the forceps, and cutting the raised part with the knife, carried horizontally. The director, introduced at this opening, was passed down towards the thorax, and on it the sheath of this vessel, near to its root, was divided.

During this, and all the subsequent steps of the operation, the internal jugular vein was removed beyond the reach of the knife by an assistant, who, with a flat silver instrument, not unlike a powder-knife, bended at the point, drew it outwards, and held it aside.

When the sheath of the carotid artery was divided, the eighth pair of nerves became visible; it lay deep, and to the outside, nearer to the carotid as they descended towards the thorax. An instrument, similar to that above described, but of larger dimensions, and greater curvature, was now employed to protect this nerve, together with the internal jugular vein. The interval between the carotid and the eighth pair of nerves in the vicinity of the thorax, was now scratched a good deal by cautious touches of the knife, for the purpose of obtaining a view of the origin and course of the subclavian artery. And now it was found that the aneurismal tumour had extended so close to the trunk of the carotid, as to leave it uncertain whether any portion of the subclavian artery was free from the disease. Indeed, so strongly were the assistants impressed with the apprehension that the aneurismal tumour had extended to the aorta, or right common trunk, that the majority appeared disposed to abandon the operation altogether. Supported, however, by the concurrence of Mr Wilmot, I determined to persevere in the attempt, at least till we could positively ascertain the actual state of the artery. I therefore proceeded to lay open that portion of the sheath which invests the very root of the carotid; in effecting which, a small artery was cut close to the coats of the carotid, and yielded a smart hæmorrhage. Some attempts were made to secure it with a ligature, but these only served to impress us with the idea that some irregular and nameless branch of the carotid had been cut so close to its origin, that it could not be tied. Our uneasiness, however, on this head was soon at an end, for the bleeding spontaneously ceased. The division of the sheath being completed, we could now see that only a small portion of the subclavian artery, lying between the aneurism and the forking of the arteria inominata, remained in a sound state. The length of this sound portion did not exceed a quarter of an inch, and on this we resolved to apply the ligature. For this purpose, I endeavoured, with the point and nail of my fore-finger, and occasionally with the flat handle of a silver director, to separate the artery and adjoining portion of the aneurismal tumour, from the parts in their vicinity. This was not accomplished without considerable difficulty and delay. But these disadvantages appeared to me of little consequence, in comparison with the danger that might attend the use of the knife in this critical step of the operation. When the artery seemed to be sufficiently detached, I took an aneurism needle of softened silver, blunt at the point, and longer than those in common use, armed it with a ligature of six silk threads, and, guiding it with my left fore-finger, passed it down on the inferior or thoracic side of the artery. But I could not push it upwards, so as to pass its point with safety beyond the cervical side of the vessel.

I then detached the artery still more completely from the surrounding parts, and again made repeated trials to pass the ligature, not only with the needle above described, but with the common aneurism-needle, and also with a loop of softened silver wire, but all without effect.

All these instruments had been introduced from below upwards, with the view of guarding against the danger of wounding the pleura.

It was now proposed to try whether the artery might not be surrounded, by passing the needle in the contrary direction. And, with this advice I complied, rather, indeed, because I had been so long, and so repeatedly foiled in my former attempts, than because the danger of the proposed plan had escaped my recollection. With the same instruments, therefore, I made a few efforts to pass the ligature in the manner recommended; in some of which I had reason to fear that the pleura was wounded, both from the increased difficulty of respiration, and from the bubbling of the air among the blood. I soon found it still more impracticable to pass the ligature in this than in the former direction. Different plans for passing the ligature were now suggested by the assistants. Among these, one was to tie one end of the ligature into a knot sufficiently large to prevent it from slipping through the eye, and then, with a sharp hook, or pair of spring-forceps, to lay hold of the knot when carried in on the concave side of the needle. I made the attempt; but the eye was so long that it allowed the knot to remain behind the vessel when the needle was pushed forward, in consequence of which, this expedient failed. After all these trials, the ligature was at length passed with a common silver aneurism-needle, introduced from the thorax upwards. But it was necessary to use a good deal of force on the occasion, so that the artery was raised up considerably out of its natural situation.

I now put a single knot on the ligature, and ventured to tighten it in the following manner: I took a polypus-forceps, the blades of which had been made fast, with an interval left between them wide enough to receive the artery. I then passed one end of the ligature through each eye of the forceps, and, holding in each hand a blade of the forceps, and an end of the ligature, I pushed the points of the forceps down on each side of the artery. By this means the ends of the ligature, which crossed each other on the surface of the artery, were drawn in opposite directions, and the knot was thus gradually tightened.

The pulsation in the tumour and in the radial artery was now considerably weakened, but not totally suppressed. Prior to tightening the noose, the breathing of the patient had become more laboured, and he complained much of an oppression at his heart. On tightening the ligature, these symptoms increased to such a pitch, that every one present apprehended his immediate death. He was raised up, and supported in a sitting posture, but without affording him any relief; his countenance grew pale, and indicative of instant dissolution. Yet his pulse did not become intermitting, or in any manner irregular; as was observed by a gentleman, who, during these awful moments, kept his finger applied to the right wrist. In this alarming state he continued for the space of about a minute, during which some of the assistants were so strongly impressed with the idea of his danger, that they quitted the room, lest he should expire before their eyes. During these struggles, the polypus-forceps was withdrawn, and, as the ligature had been tied with only a single knot, it is to be presumed that it yielded to the pulsations of the artery. Yet the patient did not appear to be at all relieved. Indeed, so great was his distress, and so strong my fears, that I had actually passed the index finger of my left hand along one side of the ligature, down to the noose, and was just about to cut through it with a probe-pointed bistoury; but, perceiving at this moment that his distress was a little relieved, the idea occurred to me that the wound of the pleura had been the principal cause of his sufferings. I now closed the lips of the wound, and this procured him immediate and sensible relief: However, the patient was so exhausted by this painful and tedious operation, and had suffered so much on the tightening of the ligature, that all present agreed to leave the ligature in its place, and to wait the issue of a few hours, or a few days. It was conceived, that, by thus allowing time for the wound in the pleura to be consolidated, the operation might hereafter be completed, as in that case, the right lung being capable of performing its office, the heart would labour less when this ligature was again tightened. Accordingly the lips of the wound were brought into close contact, by three stitches of interrupted suture passed through the integuments. The patient was laid in bed, and supported in a sitting posture, being incapable of lying horizontally.

Thursday evening, 8 o'clock.—Pulse 96, full, regular, and equal in both arms. After the operation, he had been unable to swallow any fluid, except by sucking a bit of linen wetted with his drink. Within the last two hours, has been able to swallow better, but still only a small sup at a time: Complains only of his heart; says that it feels as if a knot were tied on it; yet points to the scrobiculus cordis as the seat of his distress. Countenance rather flushed; skin moist; temperature of both hands equal. Sixteen ounces of blood being now drawn from his arm, procured him some relief.

Friday morning, 8 o'clock.—Did not sleep one moment during the night, yet says he passed it pretty well; his sister, who sat with him, says, that he had a great many catchings at his heart. What she means by this term, it is impossible for me to ascertain, as I have not seen him affected in the manner she attempts to describe. The blood drawn last night is very buffy, and much cupped; pulse this morning 120.

At 12 o'clock.—Pulse 116; distress rather less. It was determined that he should be bled again, but he was advised not to have the blood drawn till his distress should increase.

8 o'clock P. M.—He himself desired to be blooded, and sixteen ounces were taken from him at 9 o'clock. He declared himself much relieved by the bleeding: pulse only 96.

Saturday morning, 8 o'clock.—Has had some short slumbers during the night; lies much more flat and horizontal; swallows more freely, and takes large sups of his drink, swallowing it gradually; pulse 108, regular and equal in both wrists. He thinks himself much better, but still complains of his heart. No palpitation or fluttering of the heart can be felt, when the hand is applied to the thorax.

Saturday evening, 9 o'clock. Pulse 96, as he lies without speaking; when he speaks, it increases to 110. He has eaten a small quantity of boiled bread and milk, and a little cake steeped in tea: thinks himself much better. Sunday morning.—Pulse 108; has enjoyed better sleep last night; has lain on the left side for an hour, and now lies with his head less raised.

Sunday evening.—Has eaten boiled bread and milk in the day, and some flummery in the evening; lies now with his head very little raised, and has lain on both sides at different times this day. Pulse 130, and small; complains of some pain in the wound; tongue is white; countenance rather flushed; no stool since the operation.—Enema purgans statim.

Monday, 12 o'clock.—He rested pretty well last night, and feels refreshed this morning; breathing free; he lies horizontally, and on his right side; pulse 114. The enema has procured a very copious evacuation; thirst has been relieved by taking lemon juice in his barley-water.

In the interval between our first seeing him, and visiting the rest of the hospital, he was seized with a shivering, which lasted for ten minutes. During the shivering his skin did not feel cold to the hand, nor had he himself any sensation of chilliness, but said that the shivering would go off directly. He got a little warm wine, and fifteen drops of tincture of opium. His pulse and breathing seemed not at all affected by this rigor. In half an hour after the rigor had ceased, we proceeded to dress him; the lips of the wound appeared nearly united; a very small quantity only of thin discharge had flowed along the ligature. It was now deemed advisable to cut out the two lowest sutures of the integuments, and to separate the lips of the wound; for, in the present stage of the wound, this could be easily effected, whereas, if it were deferred for one or two days more, it would possibly require the use of the knife. Accordingly the sutures were cut out, and the lips of the wound separated, by drawing the flat part of an eyed probe along the line of the wound. The knot of the ligature could now be felt at such a distance from the artery, that we thought that the point of the fore-finger could just be insinuated a little between them. A consultation was now held, to decide on the propriety of immediately proceeding to tie the ligature. Some proposed that it should be altogether withdrawn; others recommended that the attempt of tying it should be made, but to postpone it for a day or two, till the effects of the rigor should have shewn themselves; while a third opinion urged the necessity of either instantly withdrawing the ligature, or tying it closely on the artery,—these last assigning as the grounds of their opinion, the danger, that the ligature, lying in contact with the artery, should so far produce ulceration of its coats, that they would tear under the ligature, if attempted to be tightened in one or two days hence; that the rigor had ceased, and the functions of circulation and respiration were going on perfectly well at this moment; in short, that the present moment was as favourable as any future period could be. The latter opinion prevailed, and the third or uppermost suture being cut out, and the wound thrown open, I proceeded to untie the remaining single knot. My object in doing this, was to include in the noose along with the artery, a bit of flexible silver wire, for the purpose of enabling us more readily to cut out the ligature, if we found, on tightening it, that alarming symptoms would be induced; another reason was, that we might tie on it the surgeon's knot, which would not slip, whereas with the single knot, the ligature could scarcely be tied tightly at the bottom of such a very deep cavity. Without much difficulty the knot was untied, a probe and aneurism-needle having been successively used for the purpose of widening the noose. The wire, very much bent, was included in the new noose, on which was formed the surgeon's knot; the ends of the ligature were passed, each through the eye, on the end of an iron wire; these were thrust down, as I then fancied, to the bottom of the wound, and attempts made to tighten the ligature, but to very little purpose. Now, fearing that the wire somehow prevented the ligature from being tightened, one end of it was cut off close to the surface of the wound, and the wire was then withdrawn, by using a slight force. Now, passing my finger down to ascertain the state of the ligature, I discovered that we had not arrived at the bottom of the wound; the wires had descended only to the neighbourhood of the anterior surface of the vessel. I now separated, with my finger, the recent attachments of the artery to the surrounding parts, and passing each end of the ligature through the eye of a polypus-forceps, the blades of which had been previously taken asunder, I passed these down to the bottom of the wound, and, giving the upper one to Mr Wilmot to hold, I held the lower one myself, and at the same time drew it very tightly. This attempt reduced the strength of pulsation both in the tumour and at the wrists; but we found that this effect was owing, in a good measure, to the artery being raised forward, and to the influence which pressing down the blades of the forceps had, on rendering it more tight. The reason why I held the blade nearer to the thorax, was, that I feared, if it had been directed downwards, the pleura must almost inevitably have been again lacerated. I next made the attempt, having only the upper end of the ligature passed through one blade of the forceps, while the lower, which, from being short and wet, could not be held firm by the fingers, was rolled round the other blade, while the fore-fingers of my right hand was employed in giving a favourable direction to the ligature, so as to prevent it raising the artery up out of its situation. After one or two attempts in this way, the closing of the artery was effected, as was manifested by the pulsation of the radial artery completely ceasing. Some slight obscure pulsation, however, was still to be felt in the tumour, which led some present to apprehend that the ligature was not drawn sufficiently tight; but, on a moment's reflection, it occurred to us, that this degree of pulsation was communicated to the tumour by the pulsation of the sound part of the artery. A small quantity of florid blood flowed from a deep part of the wound, but not in a stream, just as the ligature was tied. A second knot was now made, and the wound closed by two points of interrupted suture.

During the tying of the ligature, no alteration was observable in his countenance, respiration, or the actions of his heart. An assistant who had held his hand on the left radial artery, during the tying, could not discover the slightest alteration to have been induced by it; the patient did not exhibit the slightest symptom of pain; in fact he seemed not to suffer, in the slightest degree, from this sudden revolution effected in the arterial system; he moved the arm as freely as ever.

I saw him an hour after the operation, at which time the temperature of both hands was equal, being in both 91 degrees. He continued free from any uneasiness. He had drunk very largely of whey during the operation, which lasted one hour; his thirst at this time continues unabated; he wishes for lemonade. 8 o'clock, P. M. Monday.—Pulse 120; temperature of both arms 91 degrees; thirst continues; has not had any sleep; tongue white and dry; skin moist, and feels of the natural warmth; he did not use any food since the operation this day.

Tuesday morning, 8 o'clock.—Pulse 130, in half an hour after, 120; passed the night without sleep, and has had very great thirst, yet he thinks himself better. He wishes for a few morsels of broiled meat, rather to indulge a medical theory, than to gratify his appetite, for his stomach and bowels are extremely flatulent, which he fancies arises from want of solid food. Tongue is white; thirst is now less urgent; temperature of both arms equal.—Habeat haust: efferves. tertiis horis.

Tuesday evening, 9 o'clock.—He has just fallen asleep, and is talking almost incessantly, is tossing his limbs about, and would appear to suffer great distress; yet his sister says, that this is not different from what occurs to him when in health; for that when he first falls asleep, he talks over the occurrences of the past day, and tosses himself about in the bed, but that after he has been asleep for some time, his repose becomes quiet and profound. Pulse 120; temperature of both arms apparently the same; has had some sleep during the day, and thinks he might have had more, had he not wished to reserve it for the night; thirst is very inconsiderable since morning; eat some flummery; tongue more clean.

Wednesday morning, 8 o'clock.—He has been just disturbed from a short sleep, and appears rather confused; has passed a good night, but thinks himself weaker this morning. Pulse 120, smaller and weaker; thirst not more urgent; complains of the fetor of the discharge from the wound, and wishes it to be dressed. A considerable quantity of a yellow serous fluid came from the lowest point of the wound; part of it flowed spontaneously, and some escaped when pressure was applied to the more remote parts of the wound. The skin did not adhere to the subjacent parts; a small quantity of air seemed to be interposed. No pulsation is felt in the tumour, on the humeral side of the longitudinal incision of the neck. He appeared a good deal fatigued after the dressing. Tongue white.—Habeat pil. purg. unam statim, et post horas tres, enema purgans, nisi prius alvus soluta sit.

Wednesday, 3 o'clock P. M.—Has had some refreshing sleep, and for a much greater length of time than heretofore; has eaten five or six morsels of beef-steak, and lately a little flummery; one copious stool after the injection. The nurse reports that a small quantity of blood came off with the stool, not more than was sufficient to streak it. He is now in a quiet sleep, lying on his left side.

Wednesday evening, 9 o'clock.—Has got still more sleep; temperature of the right hand, by the thermometer, this day, 90, of the left hand 92, but the right has been a good deal exposed to the cold, being very seldom kept under the bed-clothes; pulse 120. A few minutes before this visit, a shivering had come on, which still continues, yet he does not experience the sensation of coldness, nor does his skin feel cold to the hand. His breathing now appears a little hurried, as he lies on the right side, yet, when desired to make a full forced inspiration, he does not experience any pain from it, except what he refers to the heart, or scrobiculus cordis, and this only in a trifling degree.

Thursday morning.—The rigor did not continue half an hour; he passed a good night, had one sleep for three hours, and during the rest of the night had a considerable share of sleep. Pulse 120, rather small; tossing of the lower extremities rather increased; thirst much less; discharge from the wound copious, but has passed off,—none of it has lodged. On pressing with the sponge, an unusual quantity of air escaped from under the skin; no emphysema noticed in the vicinity of the wound An obscure pulsation in the tumour was either felt or fancied by me; certainly none can be felt at the wrist; size of the tumour a good deal reduced.

Thursday evening.—Has slept well this day; has eaten more flummery than on any former day; a slight emphysema in the lower and anterior part of the neck. At five o'clock this evening some blood had been observed on his shirt, and on the bandages, yet in every respect he appears as well as usual. At 7 o'clock P. M. the dressings were removed, and the sutures cut out. On raising the flap of skin, a small quantity of coagulated blood was found in the wound, though not in its deepest part; emphysema increased; obscure pulsation continues to be felt in the tumour. Three small pieces of sponge were laid in the wound; a larger piece was laid on the skin, and retained by adhesive plaster, as pressure could not be applied by a bandage without confining the thorax too much. Pulse 120; stomach moist; tongue white; temperature of limb, and state of breathing as this morning.

Friday morning.—At 8 o'clock A. M. I received from one of my pupils the following note. "About one o'clock last night this poor fellow got a change for the worse, appeared as if strangling, with continual tossing of his legs and arms; complained of his heart, and said there was some one pulling a cord round his neck. He became quite delirious, continued in this state for nearly half an hour, after which he got comparatively easy, and has been almost ever since insensible, and is at present, I think, very near his end."

I visited him at 9 o'clock this morning. He lay insensible, rather pale, but not of that deadly paleness which so often attends great hæmorrhagies, his breathing very laboured, in short, —with every appearance of immediate death.

About 10 o'clock this morning he died.

In a few hours after his death, we hastily removed the heart and large vessels, so low down as the clavicle, being prevented by the attendance of his friends from examining the parts *in situ*. No blood was effused into either cavity of the thorax. The lungs of both sides were sound; the upper extremity of the right lung had a slight adhesion with the parts in the vicinity of the ligature. The aorta was slit up from the heart to the forking of the arteria inominata; and thus we had an opportunity of observing the very diseased condition of the coats of this artery, as manifested by the wrinkled state of its lining membrane. A very remarkable pouch or recess, capable of receiving the point of the little finger, appeared in the wall of the aorta, on the right side of the artery, about one inch above the heart.

In the apex of this dilatation the coats of the artery were extremely thin, as if, the middle coat having been removed by absorption, the lining and outer coats had adhered together. The arteria inominata was dilated at the place of its forking. Between this dilatation and the aneurism for which the operation had been performed, there was not above a quarter of an inch of the subclavian artery free from disease, and on this part the ligature had been applied. A pretty large opening had been made by ulceration, through the coats of the artery, immediately above the ligature.

The next case of disease requiring the operation of tying the subclavian artery, occurred in William Levanee, an invalided soldier, who was affected with aneurism of the axillary artery a little below the clavicle. The disease seemed to have arisen spontaneously. Tumour was the size of a pullet's egg at the time of his applying for relief.

In full consultation, the operation was decided on; but this man refused to submit to it, unless we assured him of complete success. He lingered for some months, and appeared to die, not from hæmorrhage, but rather from extensive suppuration, and long continued irritation.

Appearances on Dissecting the body of William Levanee, ætat. annos 55.

26th April 1813.—Very considerable swelling occupied the right side of the body, extending from the clavicle down to the eighth rib, and from the nipple of the breast across the arm. The arm and fore-arm were very much distended by an œdematous swelling. Three large ulcers a little below and before the axilla; inferior costa of the scapula rough on its internal surface; the inferior angle of this bone exposed and rough. On opening the thorax, the viscera were found perfectly healthy, with the exception of slight adhesions of the right lung to the pleura costalis, and some water in that side of the chest. Heart rather small, but appeared perfectly healthy. Large arteries near the heart not at all dilated, and no other traces of disease in any of them than some very small opake spots visible on their internal coats.

The pectoralis major was of a pale brown colour, and much thinner than natural; the pectoralis minor converted into a substance resembling a strong fascia. The parts above the clavicle were still in a healthy state; the different steps of the operation for tying the subclavian artery before it had reached the scaleni were performed on it. The trunk of this artery was traced of natural size and healthy texture, for an inch and a half below the origin of the vertebrals, and running over the front of the aneurismal tumour. At this place, its coats became so thin, that it was no longer possible to distinguish them from the coats of the tumour. Immediately before it became indistinct, this part of the artery seemed dilated to the breadth of a quarter of an inch. The articulation of the humerus was opened, and communicated with the aneurismal tumour. The branches of the subclavian artery did not appear at all enlarged. The basilic vein was thickened in its coats, but not at all enlarged in its diameter.

On the 19th June 1813, I was consulted by the Rev. Mr S. aged 48. He was a man of an uncommonly athletic frame; had constantly enjoyed the most robust health, and had always lived a regular and temperate life. The account he gave of his complaint was as follows: One night before Christmas last, while lying in bed, his right arm was suddenly seized with a numbness, and the right hand felt as if instantaneously enlarged to twice its natural size. Soon after this, the middle of the forearm and the insertion of the deltoid muscle were attacked with pains. He said that the physician, whom he had consulted in the country, had remarked, with some surprise, that he could not feel any pulse at the wrist of the arm affected. The disease passed for rheumatism until he came up to Dublin; but, on his arrival in town, having applied to Dr Harty to be electrified, the Doctor, during the process, observed a portion of the chain, which happened to lie on Mr S.'s breast, to be moved up and down by a violent pulsation of some subjacent part. Struck with this occurrence, he proceeded to examine the seat and cause of the phenomenon, when he discovered an aneurismal tumour situated below the clavicle, and covered by the pectoral muscle.

On the day following I saw him, and found the aneurism nearly of the size of a goose egg. The tumour pulsated strongly, but seemed, notwithstanding, to be both deeply seated and thickly covered. The pulse of the right radial artery was as full and strong as that of the left. By pressing with my thumb upon the diseased artery, above the clavicle, along the outside of the scalenus muscle, I suppressed the pulsation both in the tumour and at the wrist. But a strong pressure on the tumour itself, even when continued for some time, excited no uneasy sensation whatever, either in the region of the heart, or in any other part of the thorax; nor did it affect the powers of respiration in the slightest degree. In short, nothing preternatural could be discovered in the actions either of the heart or of the arteries, under any possible variation of circumstances.

On this very important case I requested a full consultation without loss of time; and accordingly next day Messrs Richards, Peile, and Wilmot, Dr Harty, and I, met in consultation. It was our unanimous opinion, that, although the present disease could not be clearly traced to an accidental injury, yet, as not the slightest appearance of disease could be discovered in any other part of the arterial system, it was our duty to propose the operation of tying the subclavian artery.

Mr S. who was a man of uncommon fortitude of mind, consented at once to submit to this formidable operation. He wished, however, first to return to the country for some days, in order to settle his affairs. To this delay we acceded, advising him to avoid all bodily exertions, to live on very low diet, and to debar himself of a full meal even of the coarsest fare. We also prescribed some strong purging pills, to be taken frequently, and directed that he should be blooded twice at least before his return to Dublin.

On the 16th of July he returned, a good deal reduced in flesh and colour. The aneurism had increased in size during his absence, principally in the direction towards the clavicle. Indeed, to judge by the eye only, the tumour seemed to have extended to that bone; but by the touch it could be ascertained that the tumour and the clavicle were not yet in contact. He now told us that some of his friends in the country had reminded him of his horse having fallen and rolled over him, a few days before he felt the numbness of the right hand; and he can now distinctly recollect that he suffered considerable pain about the shoulder at the time of the fall.

I commenced the operation, by an incision beginning near to the acromion, and terminating about midway up the side of the neck. Another incision, commencing from the same point, was carried along the clavicle, until it reached the outer edge of the sterno-mastoid muscle; the triangular portion of integuments included between these incisions was raised up from its apex, and laid back upon the throat. In this first step of the operation, some pains were taken to avoid wounding one or two pretty large veins which ran very superficially along the side of the neck. When the integuments were thus removed, I detached the external jugular vein, at the highest point where it lay uncovered, and with a blunt aneurism needle passed a ligature round it. I next tied up, in the same way, a branch of this vein which entered the trunk a little above the clavicle, in a direction from the acromion; and I then secured the lower extremity of the denuded trunk of the jugular vein: but to effect this last was a task of more difficulty, for the vein here was not only covered by three or four layers of tough cellular substance and fat, but was, besides, at times suddenly puffed up to a very great size, especially whenever from pain the patient made a great exertion, accompanied with a deep and long-continued expiration. But, notwithstanding all these ligatures, the trunk of the vein was still freely supplied with blood from below, as appeared by its occasionally swelling in deep expirations. This obliged me to secure the trunk with a third ligature, about an inch above the clavicle, and, having done so, I cut the vein across between the two lowest ligatures, and laid the inferior portion of it down upon the clavicle. Now removing a little fat, I saw the omohyoid muscle covered with a fascia; it was of an uncommonly large size, and lay very low, near to the clavicle; it consequently became necessary to sacrifice it, in order to get at that part of the artery which lay near to the outer side of the scalenus muscle. For this purpose, a very thin silver spatula was passed under the upper edge of the omohyoid muscle; with this it was raised a little from its bed, and was cut across. On removing this muscle, a thin fascia appeared covering a deep bed of fat. Having cut through the fascia, I now, with the flat end of a silver director, cleared my way among the fat, until I could feel, though not see, the artery. As the vessel was now lying at the bottom of a deep contracted cavity, I judged it necessary to convert this into a more open and less embarrassing form of wound. This I did by cutting across the edge of the trapezius muscle and the numerous layers of membrane which occupied the space included between the deep cavity and the acromial end of the external wound. In effecting this, one small artery was divided, and secured by ligature. When the wound was thus enlarged, the most anterior of the nerves going to the brachial plexus appeared to view. I now proceeded to lay bare the artery, holding my finger down upon it, and turning aside the superincumbent fat with the flat end of a silver director. But so great was the quantity of this substance to be removed, that, although I cut away three or four large pieces of fat with a scissars, and drew them out with a forceps, yet, to prevent the remainder from falling down again into the cavity upon the artery, I was obliged to have it held aside, by three long and broad spatulas of prince's-metal, bended at the end, and applied one to each side of the wound. This materially facilitated the subsequent steps of the operation, by keeping the artery uninterruptedly exposed to view. My next object was to detach from the surrounding parts that portion of the artery which lay close to the scalenus muscle. To effect this, I first made use of the flat end of the director, scraping with it along the outer or acromial side of the artery. I then ventured to separate it more completely, by introducing a blunt aneurism-needle in the room of the director. This I moved upwards and downwards along the vessel, until I was enabled to pass it freely under the artery, and to receive its point on the end of a narrow spatula, introduced between the vessel and scalenus. Feeling the point of the needle on the spatula, I was satisfied that it had cleared the artery. I now therefore depressed the handle. On doing so, I observed that the point was covered by a dense white membranous substance, strongly resembling the coats of an artery. I withdrew the needle, armed it with a double flat ligature, (about six threads thick,) and again introducing it on the acromial side of the vessel, I quickly showed its point by the side of the scalenus, covered with the membranous substance as before. I hesitated for a moment, but every one present being satisfied that this substance was not a part of the vessel, I readily tore through it, by scraping it with the flat end of the director. Now, merely by depressing the handle of the needle, without in the slightest degree raising the artery from its bed, I was enabled to catch the ligature on the inner side of the vessel. Having got a secure hold of it, I withdrew the needle, leaving the ligature, doubled, under the artery. I then divided the ligature into two, and tied the lower with a single knot. Mr Piele, who had applied one hand to the tumour, and the other to the right radial artery, announced, as I tightened the ligature, that all pulsation had ceased in both.

The assistants, being all anxious to satisfy themselves of these facts, I let go the ligature. Although I let it out of my hands but for the few moments I was necessarily occupied in these inquiries, yet, even during this short period, the pulsation perfectly returned; I therefore again drew the ligature tight, the pulse in the limb ceased, and I quickly made fast the ligature with a double knot. To enable us to tie this knot, we had no occasion for any instrument; nothing more was necessary than to catch the ends of the ligature close to the vessel, and then to press on them with the points of the fore-fingers, held on a level with the trunk of the artery. No other precaution was necessary to save it from being pulled up from its bed, in drawing the ends of the ligature tight. The unanimous voice of all present directed that the second or upper ligature should be tied, which was done, close to the first, with a double knot. In this awful and critical step of the operation, I kept my eye firmly fixed upon the patient. His countenance did not undergo the slightest perceptible change. No alteration was observable in the actions of his heart, or in his respiration; nor did he experience any pain or particular sensation in the limb on the tying of the artery.

It should be observed, that a large vein, which, in the dead subject, I had often found running across the artery, in a direction from the scapula towards the thorax, and which, I had feared, might be productive of much inconvenience on the present occasion, was here found to be flat and empty, and to run so near to the thorax as not to interfere in the smallest degree with the passing of the ligature.

When the operation was finished, three stitches of interrupted suture, aided by adhesive plaster, kept the lips of the wound in contact; on the flap was laid a piece of dry sponge, to bring into apposition the upper and under walls of the cavity, which had been left after the operation.

Shortly after he had been laid in bed, he complained of cold, but had no rigor. In the course of an hour, he felt himself growing too hot. In three hours after the operation, his pulse was 96. In this short period, he forced me to change his position two or three times, from his back to his sides, to try if he could by this means obtain any mitigation of what he termed a pain in the small of his back. He did not, however, complain of any pain or fulness in the head, or of any affection of the heart, or difficulty of breathing. His distress, though rather intense in degree, was of the same kind as I have frequently witnessed after amputation, or other great operations.

Mr Wilmot, who remained with him from three o'clock this day until nine o'clock at night, informed me, on my return, that his pulse had got up so high, that he had been almost tempted to bleed him; but a sweat breaking out, had reduced the pulse to its present state, (120.) For the remainder of the night I took charge of him, and, during that time, he was affected with a slight tossing of his lower limbs. He continued to complain of his back, and did not enjoy more than five or ten minutes sleep at a time. He had some thirst, and, in the course of the night, his pulse rose to 134. He did not complain of the wound, except when he turned on that side, or was awkwardly handled, and then he said it was sore. The sweat having at one time ceased for two hours, I observed that the right hand and arm became dry, as well as the left; and, on the return of the sweat, both arms were equally moist. The heat of both arms, too, appeared to be the same since the operation, and he has continued to enjoy the feelings and full power of the right hand.

Wednesday morning.—At six o'clock this morning I took 12 ounces of blood from his left arm; it flowed slowly, for he drew back his arm while I was making the incision. I gave him a draught, with two drachms of aq. ammon. acetat.; and for a short time after this, I fondly flattered myself that he became more easy and composed. These transient hopes, however, soon gave way to more gloomy apprehensions; and when I reflected on the quickness of his pulse, his restlessness, and want of sleep, his forgetfulness, and occasional wanderings of mind, I became greatly alarmed. I therefore summoned a consultation of all the gentlemen who had assisted me, for an earlier hour than that agreed upon the day before. We met at 11 o'clock A. M. His pulse was now 134, and the strokes ran so into one another, that it was difficult to count them. This state of the pulse, we judged, would not be improved by a repetition of the blood-letting. It was therefore agreed that he should be first purged, then take tincture of digitalis, and, if his restlessness required it, take an opiate at night.

Three o'clock P. M.—At this hour the physic had only operated once, and then very sparingly. His pulse was now driving at a rapid rate. He complained of an occasional cough, which distressed him so much, that he was forced to entreat the attendant to keep one hand on his head, and to grasp his left hand with the other, whenever he felt a fit of it coming on. He complained also of a soreness in his throat when he coughed or swallowed; and he pointed to the middle of his throat as the seat of this affection. Although I suspected that his fears made him magnify his distress in this particular, (for he apprehended that the inflammation, spreading from the wound to the throat, must quickly prove fatal,) yet, when I recollected that my former patient Cowell had complained of the same sensation, I deemed it prudent to meet it early, and therefore instantly commenced with the digitalis, although his bowels had not been sufficiently freed by the purging medicines.

At 4 o'clock P. M. he got the first dose of 10 drops.

At 8 o'clock the second dose of 15 drops.

At 10 o'clock 25 drops.

At 2 o'clock A. M. Thursday, 30 drops; and at 6 o'clock the same repeated.

By this medicine his pulse was reduced, so that at 10 o'clock P. M. it beat 120, but still with the same indistinctness as before.

Thursday morning, 6 o'clock.—The longest sleep he has had in the course of the night has not been above half an hour; the tossing of the lower limbs continues; they are dry, and, though covered with woollen stockings, are inclined to be cold. Countenance is natural, but more reduced than when in health. Mental faculties more perfect than yesterday. Sweat continues on the head, trunk, and upper limbs. Has had many stools through the course of the night. State of tongue and thirst as yesterday. Pulse 120, and indistinct. Moans at times to-day as he did the day before. Says that he has an uneasiness at the small of his back, but that it does not amount to pain. Thinks his throat better. There is some swelling, with a slight appearance of inflammation, across the top of the thorax. He now entertains better hopes of his recovery. He begins, however, to complain of some uneasiness in his chest, but says that it is relieved by sighing. The bandage, which feels rather too tight on the chest, is now cut across. Heat of right hand, measured by the thermometer, 94; that of the left hand 96.

Thursday evening, 8 o'clock.—This day, at 1 o'clock P. M. he was suddenly seized with a rigor, which lasted for a quarter of an hour. This was succeeded by a profuse sweat. During the cold fit he talked much and incoherently. Soon after the hot fit commenced he started up, jumped out of bed, and became so violently outrageous, that he required two strong men to hold him. In this state he continued for more than an hour. During this paroxysm he made the most powerful exertions with the right as well as with the left hand. After the paroxysm had subsided, he enjoyed a sleep of half an hour, during which his breathing was natural. The cough, which had distressed him to such a degree yesterday, is so much relieved to-day as to cause only a slight and momentary uneasiness in his chest.

Thursday night, 10 o'clock.—He had been raving a good deal at 8 o'clock this evening, but at half past nine he became quite collected, and inquired very anxiously about his own situation. Pulse more distinct, though not less frequent. Sweats continue. Some wheezing in his chest. Distress of that part less than it had been early in the evening. A large blister is applied to his chest.

The inflammatory fulness and redness of the skin at the top of the sternum and left side of the throat is considerably increased; the slightest pressure on this swelling gives him very sharp pain. Mr Wilmot and I conceiving it possible that his distress might be aggravated by some collection of matter confined or pent up within the wound, removed the dressings. Only a small quantity of reddish water was discharged. I then cut out one of the sutures, and passed a probe into the deepest part of the wound, conducting it along the ligature of the artery. We then made a pressure on the inflamed integuments, at the top of the sternum and left side of the thorax, but without obtaining the smallest additional discharge. At this time the aneurismal swelling was in appearance quite removed, and both sides of the thorax were to the sight and touch perfectly alike.

Friday morning.—He passed the night without any sleep; raved a good deal, but was not violent. Purging more frequent. Has taken two doses of a mixture, each dose containing ℥ss. oxymel. scillæ. Cough has not distressed him. He has expectorated some mucus. During the night he complained a good deal of the right fore-arm and hand,—at one time of numbness, and at another of pain. The hand is held with the fingers folded in, and rather stiffened. It now became too obvious that the storm, which had been collecting since the operation, had now taken a fatal direction. The present state of the limb so strongly proved the near approach of mortification, that, however reluctantly, I was forced to relinquish every ray of hope, and to prepare my mind for the speedy termination of a case which I had watched with more fluctuations of hopes and fears than ever had agitated me on any former occasion.

Two o'clock P. M.—The fore-arm was distended at the insertion of the pronator teres, and also where the extensors of the thumb pass over the end of the radius; the hand and fore-arm cold; the fingers rigidly flexed. The usual applications were made to the limb, but without affording any relief. The mortification went on increasing; his delirium continued for the greater part of the night; and at four o'clock on Saturday morning he died.

Appearances on examining the body 26 hours after death.

Suppuration was established under the integuments on the fore-part of the throat, anteriorly to the sterno-mastoid muscles. On the thorax being laid open, we discovered an appearance as of suppuration commencing on some masses of fat lying in the anterior mediastinum. The upper lobe of the right lungs was attached to the ribs by close and old adhesions. Between three and four ounces of water found in the pericardium. The jugular veins, both the external and internal, were of a deeper colour than usual. Heart felt rather soft; right auricle full; right ventricle less full than usual. No disease in any of the valves on either side of the heart. The internal surface of the right auricle, especially on the septum of the auricle, more red than usual. This redness, though of a brighter tinge than that of the jugular veins, was not such as to indicate the existence of inflammation of this cavity. The venous blood was more fluid than usual. Coagula were found in the right and left ventricles, but not uncommon either in their size or other properties.

On endeavouring to separate the aneurismal sac from the walls of the thorax, it was found to have connected itself so intimately with the second rib as to have its cavity laid open by an attempt to disunite them. The form of that portion of the bone upon which the aneurism rested was changed from a flat surface to a cupped form. The extent of the altered part of the second rib was about half an inch.

The aorta, with the great arteries springing from its arch, and the humeral artery, being removed from the body, were examined next day.

The aneurismal tumour had extended more in length than in breadth, and had ascended so high as to have reached near to the spot on which the ligature had been applied. The aneurismal tumour was nearly as large as a pullet's egg.

The artery immediately above the ligature was seen thrown into folds and furrows, which would seem to be the inevitable effect of tying an artery of so great a size. On removing the ligature, and slitting up the artery, its internal coat was found ruptured by the ligature only at one part, to the extent of about one-fourth of its circumference. Through the remainder of the circle the internal surface of the artery presented a white line, obviously produced by the tightening of the ligature. Two very small portions of coagulable lymph were found immediately above the ligature. The trunk of the artery below the ligature was rather small. The branches of the subclavian artery were not at all enlarged.

I have been (perhaps tediously) minute in detailing the particulars of the foregoing cases; because we have, as yet, but one account published of the operation of tying the subclavian artery after it has passed through the scaleni muscles, and no instance, I believe, has hitherto been recorded in which this artery was tied before it had arrived at these muscles. Under such circumstances, I would not presume to exercise my own judgment in omitting those particulars which to me did not seem important.

As I wished that every one who reads these accounts should be unbiassed in drawing his own inferences from the facts, I have studiously avoided interweaving any reflections of mine own. Such as have occurred to me in my attendance on these cases, I shall now briefly state.

To lay bare the right subclavian artery before it reaches the scaleni, will not be found difficult by any surgeon possessed of a steady hand, and a competent knowledge of anatomy; but I fear, that, with the utmost dexterity, much difficulty will be experienced in passing and tying the ligature, even in the most favourable cases. The instrument delineated in Mr Ramsden's book appears to me well calculated for the purpose, and yet it is not free from objection. I should fear that the coats of the artery might be cut by the sharp edges of the flexible steel plate as it is passing round the vessel. Some means similar to those I have mentioned must be adopted, to prevent the artery from being raised out of its bed while the surgeon is tying the knot.

This operation, difficult on the right, must be deemed absolutely impracticable on the left subclavian artery: For the great depth from the surface at which this vessel is placed,—the direct course which it runs in ascending to the top of the pleura, —the sudden descent which it makes from this to sink under the protection of the clavicle, and the danger of including in the same ligature the eighth pair of nerves, the internal jugular vein, or the carotid artery, which all run close to and nearly parallel with this artery; these all constitute such a combination of difficulties, as must deter the most enterprising surgeon from undertaking this operation on the left side.

Even on the right side this operation will be very seldom required; for it can only be called for in injuries or diseases of that small portion of this artery which lies between the scalenus and the clavicle. It is scarcely necessary to observe here, how frequently such a case will be complicated with diseases of the great trunks of the arteries nearer to the heart, or how difficult it must be to discover the existence of those internal affections. When examining any pulsating tumour near the top of the thorax, the surgeon should bear in recollection the remark made by Mr Astley Cooper, as quoted by Mr A. Burns, in his Surgical Anatomy of the head and neck, " that aneurism of the aorta may assume the appearance of being seated in one of the arteries of the neck." The quantity of blood lost by the hæmorrhage, which yet we presume was the immediate cause of death in this case, was very inconsiderable, being infinitely less than I had ever before known to prove fatal.

The operation of tying the subclavian artery on the acromial side of the scaleni muscles is one which will be much more frequently required, and which can be performed with equal facility on the artery of either side. The most striking and most unfavourable circumstance of this operation is, the great length of time required to perform it, although the ultimate and essential steps of it had been executed with so much facility.

This delay is by no means unavoidable. Indeed, any man who will take the trouble of reflecting on the anatomy of the parts concerned, or who will himself perform this operation on the dead subject, must be convinced that it may be executed in as short a time as the operation for femoral aneurism. The causes of delay in this instance were, first, the almost unprecedented nature of the operation; the caution with which we tread upon untried ground rendering every incision more slow, and every step more deliberate. Next, the great anxiety to secure every vessel, even the smallest branch of an artery, or a vein, lest our view should be obscured, or our progress obstructed by any quantity of blood flowing into the wound, was a principal source of this delay. By referring to the account of the operation, the reader can readily estimate what loss of time was occasioned by this object. I do not mean to say that we should pass through the various stages of the operation absolutely regardless of the flow of blood; but I am confident that we have no occasion to tie up any but the larger veins, and the suprascapular artery, if it should chance to be wounded. The curved spatulas, while they hold aside the lips of the wound, will at the same time serve to stop the bleeding. I cannot forbear to recommend, in the strongest manner, the use of these instruments, not merely in this, but in every other operation where the depth of the wound is considerable, compared with its length, and where it is of importance for the surgeon to distinguish the parts which lie at the bottom of such a cavity.

The necessity for removing any portions of fat can occur only in very corpulent subjects.

In passing the needle round the artery, it will be found absolutely necessary to have the scalenus muscle held back by one of these spatulas, otherwise it will be impossible to pass the ligature without including along with the artery some portion of the muscular fibres.

The facility with which the ligature was passed round the artery was in the highest degree gratifying to every one present. This inestimable advantage will be secured by an attention to the following points: First, To extend the wound out towards the acromion, by which the form of it is changed from a deep cavity to a superficial wound; next, To introduce the needle on the outer or acromial side of the artery; and, lastly, to select the most favourable part of the artery. This, on inspection, will be found to be where it has just passed through the scaleni. How necessary this selection is, will appear by a perusal of the account of Mr Ramsden's operation; for he, by attempting to secure this vessel near to the first rib, or rather, as he says, at the lower edge of the first rib, found it almost impossible to turn the needle round the artery in the very narrow space between this bone and the clavicle.

Cases in which this operation may be necessary, will not be very unfrequent. In wounds of the axillary artery, either while it runs in front of the thorax, or while it lies along the humerus, this operation will be preferable to following the course of the wound by cutting through the pectoral muscles in the one case, or entangling ourselves in the brachial plexus, when the artery is wounded in the axilla. The pain and difficulties of the operation above described are trifling, when compared to those which must occur in following the course of these wounds.

When an aneurism of the axillary artery shall require this operation, we may indulge a confident hope that the rest of the arterial system is free from disease, as it appears to have been in two of the foregoing cases, that of Levanee, and of the Rev. Mr S.; one, where the disease arose spontaneously,—the other, where it could be traced to accidental injury. Although this operation has not yet proved ultimately successful, yet I think we should not despair. The history of surgery furnishes parallel instances of operations, now generally adopted, which, in the few first trials, failed of success.

Stephen's Green, Sept. 6, 1814.

II.

Observations on the Fœtal Liver, &c. &c. By James Bryce, F. R. S. E. Fellow of the Royal College of Surgeons Edinburgh.

IT is a circumstance known to all anatomists and physiologists, that the size of the liver, compared with the other viscera, or with the body, is much greater before birth, than after that period; and, although the immediate cause of this be readily explained, by comparing the anatomical structure of this organ during the fœtal state, with its structure in the adult, yet, for what purpose it is intended by nature that this peculiarity should constantly take place in the fœtus, seeing less bile is secreted by the liver before than after birth, is a question which, as yet, has not been explained to the satisfaction of physiologists.

In the following pages I shall attempt to explain some important advantages which the constitution of the new-born infant obtains from the diminution which takes place in the size of the liver immediately after birth, and which, I presume, will appear to be of such consequence to the animal economy at that critical period, as to entitle me to deduce from them the final cause, or the purpose intended by nature to be effected, by the greater size of that organ during the fœtal state. For this purpose, we shall first attend to the general situation and connections of the liver, and, from a comparative view of its anatomical structure in the fœtus with that in the adult, point out those circumstances on which its great size before birth seems immediately to depend.

The liver is the largest of all the abdominal viscera, and also the largest gland in the body. It is of a very irregular figure, being convex and smooth above, concave and uneven below. It is situated in the upper part of the abdomen, having its smooth and convex surface contiguous to the arch of the diaphragm, and its concave surface in contact with the stomach and part of the intestinal canal. In the adult, its size is such as, in a healthy state, to be easily contained in the right hypochondrium and epigastric regions; but, in the fœtus, it not only fills these completely, but also the left hypochondrium and greatest part of the umbilical region. It is divided into lobes, which, from their situation in the abdominal cavity, have acquired the names of right and left, by a deep furrow on the inferior surface, and by a corresponding membranous ligament above. In the fœtus, these lobes are nearly of an equal size; but, in the adult, the right is much greater than the left. Besides this division of the liver into two great lobes, there is, situated upon the right lobe towards its back part, and near the deep furrow already mentioned, a triangular eminence, called, by anatomists, the lobe of Spigelius; and near it another smaller eminence which is anonymous, but to which and the other now mentioned the name of portæ has been applied, because between these pass the blood-vessels of this organ before entering its substance.

The liver is connected to the surrounding parts by means of ligaments, which are formed chiefly by doublings of the peritonæum. The most conspicuous of these is that already mentioned as corresponding with the deep furrow. It is called by some, from its shape, falciform; by others, from the office it seems to perform, suspensory. By this ligament, which, in the fœtus, contains the umbilical vein in its duplicature at the lower edge, the liver is connected with the umbilicus, the anterior side of the abdomen, and with the diaphragm. By doublings of the peritonæum are formed the two lateral ligaments, connecting the lateral portions of the liver with the corresponding parts of the diaphragm. And another connection of this viscus, which has improperly obtained the name of ligament, is merely an adhesion of its substance to the tendinous part of the diaphragm.

The liver, even in the adult, is supplied with blood in a very ample manner in proportion to its bulk.

The great trunk of the aorta, having passed through the diaphragm, sends off a large vessel, called cœliac artery, which soon divides into three branches; one of which is distributed on the stomach, and another, passing to the left, goes to the spleen, while the third and largest goes to the right, and, giving off some branches to the adjacent parts, carries blood to the liver, and is then called the hepatic artery. This artery, passing between the portæ, enters the liver, and is, by innumerable ramifications, distributed throughout its whole substance, terminating in the beginnings of corresponding veins. The blood which this artery carries to the liver, as possessing all those qualities fitted for the nourishment of the animal body, is supposed to be destined solely for that purpose with respect to this organ.

Besides the blood thus carried to the liver by the hepatic artery, it also receives, in a manner peculiar to itself, a large quantity of blood by the vena portæ,—a vein apparently performing in the liver the office peculiar to an artery in other parts of the body. This vessel begins by innumerable small branches, and receives the blood from the whole of the other abdominal chylopoietic viscera. These branches unite, in their passage towards the liver, into one great trunk, which, together with the artery already mentioned, is inclosed in a membranous capsule, and, running between the portæ, enters the liver, and, like the artery, is distributed throughout its whole substance. Of the ramifications of the vena portæ, some terminate in corresponding hepatic veins, whereby the blood is conducted into the vena cava inferior, and some in another system of vessels, called biliary ducts, through which the bile is carried from its secerning puncta into the gall-bladder and intestinal canal. The blood carried to the liver by the vena portæ is, from circumstances in the mode by which it is conducted thither, and circulated through it, and from its nature and general appearance, deemed that alone from which the bile is secreted by the peculiar action of this organ.

Besides these vessels, which are common to the fœtus and the adult, the liver receives before birth a very copious supply of blood by the umbilical vein. This vessel, conveying the blood from the placenta, passes into the body of the fœtus at the umbilicus, enters the substance of the liver at the top of the great tranverse cleft or furrow which divides it into two lobes, and immediately detaches very considerable branches, by which it not only supplies the left lobe almost entirely, but affords also a very considerable supply to the right. Hence we find the umbilical canal, which is justly esteemed the termination of the umbilical vein, scarcely equal to one-fifth part of the size of that vessel before it enters the substance of the liver. Seeing, then, that the whole blood passing from the placenta to the fœtus passes through the umbilical vein, and that at least four-fifths of it must circulate through the ramifications of that vessel in the substance of the liver before it enters the inferior cava, we readily account for the greater size of that organ before birth, (viz. from the vast mass circulating through it,) than soon after that period. Hence, also, we see how the size of the fœtal liver, as depending on the increased quantity of blood which it receives by the umbilical vein during the fœtal state, must very quickly be diminished after birth, when the supply by that vessel is interrupted. And hence, also, we observe why the diminution, which takes place in the size of the liver after birth, is chiefly in the left lobe.

As another immediate cause of the greater size of the liver, during the fœtal state, may also be mentioned the presence of much mild and viscid bile in the biliary ducts, the ramifications of which extend throughout its whole substance. This is evident by anatomical inspection; and there is no doubt that, from this cause, the magnitude of the liver must be very considerably increased, compared to what it will be afterwards, when, from the action of the muscles of respiration, &c. this viscid matter comes to be completely evacuated into the intestinal canal. I would here observe, that there are many circumstances which render it probable that this mild and viscid bile, which is collected in the biliary ducts of the fœtal liver, by being propelled from thence into the intestinal canal immediately after birth, constitutes the meconium of new-born infants. It seems difficult to understand how the meconium, if secreted from the glands of the intestines during the fœtal state, or if propelled rom the stomach during that period, should lodge in the bowels several months without inconvenience, and should, all at once, immediately after birth, acquire such a degree of acrimony as to induce those violent symptoms we daily observe to take place in newly-born children from the retention of this matter. The voiding of the meconium is evidently intimately connected with respiration; for an infant born in the seventh month will void this viscid matter in a few hours after that important function is established; and there is no instance in which infants born at the end of the ninth month have ever suffered this evacuation previous to their birth. As the evacuation of the meconium, then, is as common to the infant born at the beginning of the seventh month as to that born in the end of the ninth month, it is evident that this matter is equally prepared for evacuation in the seventh as in the ninth month, although, perhaps, not in the same quantity; but how can we suppose this matter to be lodged in the intestinal canal with impunity, during two months before birth, seeing the retention of it for a single day after birth, whether that happens in the seventh or in the ninth month, is constantly attended with much inconvenience?

The explanation of the origin, and of the excretion from the newly-born infant, of that viscid and tenacious matter which is called meconium, seems to be this: By the peculiar action of the liver during the fœtal state, much mild and viscid bile is secreted and accumulated in the biliary ducts throughout its whole substance. By the action of the diaphragm, and other muscles of respiration, immediately after birth the liver is considerably compressed, whereby the viscid bile, accumulated in the biliary ducts, is propelled from thence into the intestinal canal, where, acting as a powerful stimulus to the tender coats of the intestines, it excites them into action, and is consequently carried downward, and evacuated from the body; or if, from accidental circumstances, it be retained in the intestinal canal, it then induces those troublesome symptoms which are known to arise from acrid matters applied to the tender coats of the intestines. Does the pure arterial blood derived from the placenta, and which circulates through the fœtal liver, by imparting oxygen to the blood in the vena portæ, render the blood in it less fit for the secretion of bile? and also, does it render what is secreted from it less acrid? If so, ought not advantage to be taken of a knowledge of this circumstance to relieve, or effectually to remove, those violent symptoms which are induced by a superabundance of bile, or by a particular acrid state of it, by throwing in this principle of oxygen largely into the alimentary canal, whereby the blood, passing from thence to the liver in the vena portæ, may become highly impregnated with it? Is it on this principle that acids are esteemed correctors of acrid bile?

The final cause, or the purpose intended by nature to be effected by the great size of the liver in the fœtus, compared with what it is soon after birth, is a question which still remains unexplained to the satisfaction of physiologists. "Huccusque," says a late author, when treating of this subject, "nulla sufficiens assignatur ratio, ob quam vena umbilicalis ad hepar potius quam ad cavam immediate feratur, cur canalus venosus sit umbilicali angustior. Si vero insignem hepatis molem in fœtu spectemus peculiari usui hoc viscus inservire, suspicio oritur, licet nondum sit nobis manifestus." * And in a similar manner do other physiologists express themselves on this subject, marking this peculiarity in the fœtus as a circumstance in the operations of nature highly deserving of attention. †

How far the following explanation of certain advantages which result to the newly born infant from the diminution which takes place in the size of the liver immediately after birth, may appear satisfactory to account for its greater size during the fœtal state, I must leave to the able physiologist to determine. To me it appears that such a state of that organ, and effected precisely by the means we have mentioned, is so necessary in order to complete that grand revolution which takes place in the circulating system at birth, that any deviation from it, would, in my opinion, at that critical period, be attended with certain and immediate death.

In order to illustrate this, I would observe,

1*st*, That during the fœtal state the lungs are completely collapsed, are of a firm and compact texture, having very little if any ‡ blood circulating through them in the pulmonary artery, and that there is no empty space in what is called the cavity of the breast.

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* Vide Scassi de Fœtu, p. 93.

† Vide Monro's Lectures, MS.

‡ It appears to me extremely probable, that no blood circulates in the pulmonary artery and veins during the fœtal state, but that the whole of the blood passes directly from the right to the left side of the heart through the oval opening; that the right side of the heart and the arterial canal are not called into action until the period of birth, and that then the use of the arterial canal is only to convey a certain quantity of the blood from the pulmonary artery into the aorta, which could not be transmitted through that vessel until, by the full dilatation of the lungs, a free passage for the blood was obtained during all periods of respiration, after which this canal becomes shrivelled, and is gradually obliterated.

2d, That in the fœtus the muscles of respiration are in a relaxed state, or in that state to which they will naturally return after every contraction, or at the end of every natural expiration.

This is strictly true with regard to the intercostal muscles; and with regard to the diaphragm, I would observe, that that muscle is not only not in a state of contraction, but that it is pressed upwards considerably beyond the state of natural relaxation, which occasioned the Baron de Haller to observe, that "the cavity of the breast is short in the fœtus, and greatly depressed by the enormous size of the liver."

3d, That after the function of respiration is established in the newly born infant, a certain quantity of air remains in the lungs after every natural expiration.

This is proved by the simple experiment of immersing the lungs of an infant that has breathed, in water; although, before the infant breathed, the lungs were so compact as quickly to sink in that fluid, yet, after having breathed, the air can never again be so completely expelled from them by the ordinary efforts of respiration, as to render them again heavier than water, and they, therefore, now swim in that fluid. From experiments it is known, that a considerable quantity of air remains in the lungs of an adult after the most complete natural expiration, and that a quantity proportionally great is constantly in the lungs of the newly born infant, after the function of respiration and the great revolution in the circulating system have been fully established, there seems little room to doubt.

4th, This quantity of air remaining in the lungs after every complete natural expiration, and occasioning what I would term *the permanent dilatation of the lungs*, is absolutely necessary to admit of the free circulation of the blood through the pulmonary artery, during that period of respiration which takes place between expiration and inspiration.

This must appear evident, when we recollect that very little if any blood passes through the pulmonary artery in the fœtus, and also when we attend to that train of symptoms which clearly mark a disturbed, or entirely interrupted circulation in the pulmonary vessels, which takes place from diminishing this permanent dilatation, either by compressing the lungs themselves, as in hydrothorax, and in wounds of the breast, when air is admitted between the lungs and the pleura lining the ribs, or by diminishing the natural capacity of the chest, as happens by fits of coughing, or laughing, or from an increased size of the liver itself, as in many cases of hepatitis.

On an attentive consideration of these circumstances, the following question will naturally suggest itself: Whence proceeds, in the newly born infant, such a space in the chest, as to allow of that degree of permanent dilatation of the lungs, which is necessary for the free circulation of the blood throughout the pulmonary vessels during all periods of respiration, more particularly during that period which takes place between expiration and inspiration? This question can only, I apprehend, be properly explained by carefully attending to the changes which take place in the liver immediately after birth.

From the situation and connections of the liver, formerly explained, we readily understand how any change in its magnitude will occasion a change in the capacity of the chest; and we know that a chief cause of the great size of this organ, in the fœtus, is the great quantity of blood which it receives by the umbilical vein; and it is evident that when this supply is cut off, as happens at birth, the size of the liver must necessarily be much and quickly diminished. But this dimunition in the size of the liver will necessarily remove that degree of pressure from the diaphragm, which occasioned it, in the fœtus, to protrude into, and greatly diminish the capacity of the chest. From this circumstance, the lungs will admit of a greater degree of expansion, from the air which is taken in by inspiration, and a certain degree of permanent dilatation of them, in proportion to the dimunition of the liver, will be established, independent of the muscles of respiration. Thus, by the action of the muscles of respiration, by whatever means this may be excited, a quantity of air is taken into the lungs immediately after birth, which, if the circulation in the umbilical cord remains entire, will be completely expelled again at the end of every expiration; but as the force of the circulation in the cord, and consequently the size of the liver, quickly diminishes, the function of respiration becomes more strong; the capacity of the thorax, and the permanent dilatation of the lungs, increase; and as, in a short time, the circulation in the cord becomes entirely interrupted, the size of the liver, as depending on this circumstance, must be greatly diminished, the capacity of the chest increased, and the permanent dilatation of the lungs so far established, as to allow the blood to flow more or less freely through the pulmonary vessels at all periods of respiration; and, in proportion as the inspirations become more deep, and the muscles of the thorax and abdomen act more strongly, the size of the liver will be still farther diminished, by being more fully emptied of the remaining blood which it had received by the umbilical vein, and also of that viscid bile which was collected in the biliary ducts in the fœtal state: thus still farther increasing the capacity of the chest, and establishing such a degree of permanent dilatation of the lungs as freely to allow the passage of the whole mass of blood through the pulmonary vessels, even after the deepest natural expiration; at which time the arterial canal will become flaccid, and begin to be obliterated, and the revolution in the circulation of the blood may be reckoned complete.

The great advantages which the animal constitution obtains from this diminution which takes place in the size of the liver after birth, and consequent establishment of a considerable permanent dilatation of the lungs, are evident throughout life. By this means, the important function of respiration is rendered far less liable to be impeded, or entirely interrupted, by those accidents to which we are daily exposed. Thus, the smallest degree of diminution of the cavity of the thorax, or pressure on the lungs, as from coughing, laughing, from hydrothorax, empyema, &c. must, but from the circumstance mentioned, have been attended with very great inconvenience, or even immediate death; whereas, by the permanent dilatation of the lungs being established, we find, from experience, that a very considerable degree of diminution of the cavity of the chest may take place, or that the above diseases may be present to a considerable extent with little inconvenience.

The Baron de Haller, when treating of the peculiarities of the fœtus, more particularly of the great size of the liver, and of the diminution which it undergoes after birth, says—"Diminutio hepatis est ex naturæ finibus, locum enim pulmoni parat in quem se distendat, et obstaculum minuit quod diaphragmatis per inspirationem descensui se opponebat."

Here it is evident that there is a very great difference between the advantage supposed by the Baron de Haller to arise from the diminution of the fœtal liver after birth, and that which I have now explained; because, although we agree that the cavity of the chest is thereby increased, yet the opinion of that celebrated author was, that, by this diminution in the size of the liver, the lungs were merely allowed to be dilated more freely at each inspiration than they could otherwise have been: but he certainly does not state, that, from this circumstance, such a degree of permanent dilatation of them is established as to allow the blood to flow freely through the pulmonary vessels during all the periods of respiration. Nay, it appears very clearly, that of this permanent dilatation of the lungs, and of the free circulation of the blood through them in the pulmonary vessels, during all the periods of respiration, he was entirely ignorant, as may be collected from the following passage, contained in his celebrated work: "In inspiratione summa facilitas nascitur sanguini de corde dextro exeunti. In expiratione vero pulmo undique urgetur et in multo minorem molem comprimitur: vasa ergo sanguinea breviora quidem fiunt cum retractis bronchiis eademque angustiora nunc sunt, siquidem pectus secundum tres suas dimensiones arctatur. Sanguis ergo quidem in pulmones undique comprimitur; et venosus æqua vi pressus, partim versus arteriosum quidem reprimitur eumque moratur aliquantum, partim versus cor sinistrum promovetur. Quare in expiratione quam ponimus stabilem superesse, pulmonis pro sanguine immeabilitas oritur, quam neque absque palpitatione et vitioso conatu, demum omnino ullis suis viribus cor vincere queat." Lib. viii. sect. 4.

Besides the permanent dilatation of the lungs, there is also another advantage which the animal constitution derives from the diminution which takes place in the size of the liver immediately after birth, which is perhaps not less important. Since, during the fœtal state, very little if any blood circulates in the pulmonary artery and veins, and as, after birth, a very considerable quantity is constantly present there, it must be evident, that, unless some supply was added to the circulating mass, at the critical period when the revolution in the circulation takes place, such a quantity of blood must necessarily pass from the aortic into the pulmonic circle as to induce, in the former, such a loss of tension and tone as would be attended with the very worst consequences. In order, therefore, to prevent this, it is wisely ordained by nature that the liver should, in the fœtal state, receive, in the manner we have formerly pointed out, a much greater quantity of blood than is necessary for it to retain after birth. From this circumstance, when, at birth, the circulation in the cord comes to be interrupted, the great quantity of blood which is at that time contained in the vessels of the liver is propelled into the vena cava, whereby such a supply of that fluid is given to the general circulating mass as is necessary to prevent those bad consequences which would otherwise have arisen from the passage of so considerable a portion of blood from the aortic into the pulmonic circle: so that, in proportion as the liver is, by the action of the muscles of respiration, &c. gradually diminished in size, will not only the permanent dilatation of the lungs be increased, but also will the quantity of blood which is, after birth, constantly found in the pulmonary vessels of the lungs, be supplied by that which is propelled from the liver; and by this transmission, as it were, of a certain quantity of blood from the liver into the newly-dilated lungs, will that due balance be supported throughout the whole circulating system, which is so necessary for health, and that loss of tone and dangerous debility prevented which would, at such a critical period, have been attended with faintings, or the immediate death of the tender infant.

From these observations, I conclude,

1*st*, That the immediate cause of the great size of the liver, in the fœtal state, is chiefly owing to a greater quantity of blood circulating through it before birth than after birth.

2*d*, That the diminution which takes place in the size of the liver, after birth, is effected by the change which takes place in the circulation of the blood at that period.

3*d*, That from this diminution in the size of the liver, after birth, arises *that degree of permanent dilatation of the lungs* which is necessary to admit of a free circulation of the blood through the pulmonary vessels, during that period of respiration which takes place between expiration and inspiration.

4*th*, That, *by the transmission of a quantity of blood from the liver into the newly-dilated lungs, at the period of birth*, that loss of tone and debility is prevented which would often be attended with fatal consequences.

5*th*, That these circumstances are of such important advantage to the animal constitution as to ensure the constant formation of the greater size of the liver before birth than after that period, and are consequently to be considered as the purposes intended by nature to be effected by that peculiarity in the fœtus.

III.

Reply to the Review of Mr Baynton's Essay on the Cure of Crooked Spine, in No. 39. of this Journal. By HENRY EARLE. Surgeon to the Foundling Hospital, and to the Central National School.

HAVING been educated in a school where the beneficial effect of counter-irritation is inculcated, and having enjoyed ample opportunities of witnessing the most happy results from the judicious application of issues in counteracting and arresting the progress of the most formidable diseases, I was not a little astonished at reading the philippic against their use, in the critical analysis of Mr Baynton's treatise, contained in your last number.

Anxious to ascertain on what grounds the reviewer had attempted to overthrow a practice which has met with the sanction of the most enlightened men in the profession, I hastened to procure Mr Baynton's work, and have perused, with attention, that gentleman's more "humane" plan of treatment, which has drawn forth such warm encomiums. Considering it as an indisputable axiom in our profession, that we must often "be cruel only to be kind," and conceiving that to be a very mistaken species of humanity, which, rather than inflict a temporary evil, suffers a baneful malady to proceed without any interference, I must hesitate in admitting this epithet, until I am convinced of the superior efficacy of the plan in question. On mature consideration of all the evidence adduced by Mr Baynton, my opinion is so decidedly opposite to that of the gentleman who has reviewed his work, that I must beg the admission of the following observations, in defence of issues, in your next number.

In thus standing forward as the champion of a long-established practice, which, I am happy to say, rests on higher authority than my testimony, nothing is farther from my wishes or intention than to enter the lists of paper controversy. But, knowing that many persons, either from want of time to read the original works, or from the high opinion they entertain of the reviewers, are apt to fix their faith on such critical analyses, I am unwilling to suffer the impression, which the review of Mr Baynton's work cannot fail to make, to remain without some attempt to obliterate its injurious effect. I am farther induced to take up the present subject, as it is one of great practical utility.

In answering an analysis of any work, I know no better method of preventing useless discussion, than to make a counter analysis and examination of all the facts adduced on the opposite side. I shall proceed, therefore, to take a cursory view of the subject of Mr Baynton's treatise, and then carefully examine the individual cases which he has published in support of his practice.

He begins with some observations on the dreadful consequences of the disease, and extracts a history of the symptoms from Mr Pott's works. He assents, in general terms, to the opinion of that author, that the disease of the vertebræ is dependent on a scrofulous habit. He next takes a view of the difference of opinion between Mr Pott and Sir James Earle, respecting the advantage of employing machinery in combination with issues and setons; and he conceives "that greater advantages may in future be expected from the use of means that are more simple, more reasonable, and much more agreeable." He next says, "that diseases of the vertebræ, which, in consequence of the softness of the bodies, occasion the protrusion of the spinal processes, and a *compression of the medulla*, must be generally allowed to be connected with the disease that is denominated scrofula." I extract this passage merely to make an exception to the term "compression of the medulla," which certainly does not necessarily take place until the very last stage of the complaint. I shall have occasion to resume this subject when speaking of the peculiar symptoms of the disease. Here follow some speculations on the nature and cause of scrofula, which he refers "to a debilitated state of the arteries, veins, and absorbent glands and lymphatic vessels." In another place, he observes, that "these effects may be occasioned either by a deficient deposition of ossific matter from the secreting extremities of languid arteries, or by a deficient absorption of the softer constituents of bone by diseased absorbent vessels, or by the combined effects of each of these, and perhaps of other causes." I would willingly pass over in silence this more visionary part, but that the author, in a subsequent part, founds his practice, in great measure, on this theory, and says, "that resting in the horizontal posture is as effectual in improving circulation, favouring the deposition of bone, and promoting absorption, as it is in preventing pressure and allaying pain."

It is not my intention to enter into the probable remote causes of scrofula; and I shall content myself with observing, that there is more evidence of a diseased action of vessels which deposit a carious substance in lieu of bony matter, than of any languor in the arteries, &c. In the analogy which follows between diseased vertebræ and a broken bone, he fails completely in resting his opinion on debilitated vessels; for it is well known, that often when a bone is broken in a person whose circulation is languid, and bodily powers feeble, the fracture will not unite firmly, unless some slight degree of motion is occasionally resorted to during the progress of the cure. In objecting to this analogy, I must beg to be understood not to deny the expediency of taking off pressure from parts which, from their softened state, are incapable of bearing any weight. In this respect I perfectly coincide with Mr Baynton; but I much doubt if rest alone will stop the progress of the complaint, and counteract the diseased action of vessels. Mr Baynton conceives, that, because in many of Mr Pott's cases the patients were kept in a horizontal posture during part of their cure, the beneficial results are wholly referable to rest. As the facts on which he grounds this assumption can alone guide us in deciding on the validity of his doctrine, I shall proceed to examine, individually, the proofs which he has adduced.

The first case will not detain me long. It was an affection of the hip-joint, which was treated with setons, combined with rest, during the whole progress of the cure. This I think cannot, by any impartial reader, be considered in any way conclusive, as the cure may with as much, if not more, reason be attributed to the counter-irritation.

Precisely the same observations will apply to the second case of diseased vertebræ, in which also setons were had recourse to during the whole treatment. The third case proved fatal before he could apply his mode of relief; but Mr B. flatters himself, that a perfect recovery might have been obtained by absolute rest alone, if it could have been submitted to. I think that I may venture to say, that he would have been greatly disappointed in his expectations. This third case is adduced as an instance of the failure of drains. On a careful review of all the circumstances of this case, I cannot think that their inefficacy, in the present instance, can be fairly attributed to their want of power, unless it can be proved, that the real value of a remedy can be lessened by the abuse of it. It appears that the subject of the case, a Mr Kennedy, "had nearly lost the use of his lower limbs in consequence of a curvature of the spine, that had occasioned it to take the shape of a half bent hoop. *No one of the vertebræ projected more than another, but the whole spinal column had assumed a shape similar to that which is occasioned when a person stoops moderately.*" Mr B. was directed to what he conceived the seat of the malady, by a degree of tenderness on pressure of many of the superior dorsal vertebræ, and here he applied setons eight or nine inches in length, which were kept open for eighteen months without the smallest advantage. It would indeed have been surprising if they had produced any good effect; as I very much doubt whether the disease existed at all in the vertebræ; and even supposing that they were the seat of the malady, in what respect does the history and progress of the complaint at all correspond with the history of disease in the bodies of the vertebræ? In what, therefore, does it militate with the practice recommended by Mr Pott for that peculiar affection? It is necessary here to bear in mind, that Mr B. sets out with extracting the symptoms from that author's valuable treatise, and then proceeds to relate cases perfectly dissimilar from the characters which he has laid down; which he, Mr B., brings forward as proofs against the treatment. I would ask here, if Mr Baynton, or any other surgeon, ever met with the affection described by Mr Pott producing this gradual stooping of the body? And whether he ever knew a case of scrofulous caries of the vertebræ, where the extent of the curvature was not more or less defined? I would further ask Mr B., *whether, in the present case, when the patient was placed in a recumbent position on a flat surface, the vertebral column was not spontaneously restored to its proper form?* I have said that I do not consider this as a case of diseased spine. My reasons for adopting such an opinion, arise from having met with two cases, precisely resembling the one in question in their history and progress, the particulars of which I will briefly relate.

Captain C., aged 36, applied to me, July 26th 1811, in consequence of a palsied state of the lower extremities, of which he gave the following history: That he had always been remarkably healthy and strong until about four years before, when one day, after having undergone great fatigue, and been wet through for many hours, he had a slight attack of fever and headache, and the following day, on attempting to walk, he found a stiffness in his legs and an inability to move. In a few days he was incapable of expelling his urine, and his feces, at times, passed involuntarily: That these symptoms gradually increased, until the lower half of his body was completely palsied: That he had tried blistering, friction, Bath waters, and a variety of remedies without any alleviation: That he had been in this state for upwards of three years, his health gradually declining, when he went to reside by the sea-side, where he met with Mr Pott's treatise; and conceiving that his case resembled the disease described by that author, he caused two issues to be made on either side of the middle dorsal vertebræ. These were kept open for several months without his experiencing any decided benefit in his limbs. His general health was improved by the sea air. Almost in despair of ever recovering the use of his limbs, he now came to London and placed himself under my care. When I first saw him, about four years from the commencement of the disease, he was in a most helpless state; his lower extremities were cold and benumbed, and he had lost all power of directing his steps. His limbs were not, however, perfectly palsied, as he was still sensible of any severe injury or pinch; the muscles were rigidly contracted, and toes pointed to the ground, as described by Mr Pott. The rectum and bladder had nearly lost their power. The whole vertebral column was slightly arched in the form of a half hoop, as in the case described by Mr Baynton, but no one part was unnaturally prominent. His general health was better than it had been for years; but his bowels were torpid, and required the frequent exhibition of purgative medicines, at which times the rectum was incapable of retaining its contents. From the circumstance of not being able to decide at what part of the spine to apply the remedy, though, at the time, I certainly considered the symptoms to depend on some disease in the vertebræ, or medulla spinalis, I could not afford him much prospect of relief; but as the issues which had been formerly made were very small, and had not been properly kept open, I complied with his request, in making fresh drains. Two large caustic issues were made, at some interval from each other, in the lumbar region. The sloughs were six weeks before they separated, and the ulcers required daily stimulants. He persevered in their use for six months, without any visible amendment. I now desired him to heal up the issues, and recommended friction and electricity. From these he derived very considerable benefit; his legs became much warmer, and more sensible of impressions; and, with the assistance of leaning on the back of a chair, he was able to walk across the room, by throwing the limbs forward by an effort of the whole trunk; but he was yet unable to direct his steps. When supported against a table or a wall, he could raise his body from its arched form to a proper upright position. This, indeed, he was able to effect when I first saw him, but with a greater effort; and when he was in a recumbent position, the form of the back was quite natural. This circumstance, contrasted with subsequent experience, induces me strongly to believe that there was no disease whatever in the vertebral column; and this arching of the body was purely the effect of deficient muscular action, rendering the muscles of the back incapable of supporting the weight of the head and trunk. And, from the result of the examination which I was enabled to make, a short time after, in a case which I shall presently relate, I am firmly of opinion that the deficient nervous energy in this case depended either on some morbid affection of the brain or its membranes, which rendered it incapable of transmitting its influence to the extreme parts of the body, or some diseased affection of the medulla spinalis, wholly independent on any disease in the vertebræ. About this time he was obliged to leave London, and I have never since heard whether he continued to derive benefit from the use of electricity, or remained in the same helpless state.

The next case, which occurred in a soldier between 30 and 40 years of age, was so precisely similar in its history and symptoms, that I should perhaps incur the charge of prolixity if I were to enter into all the circumstances minutely. In this case, superadded to the curved back, and deficient muscular power in the lower limbs, there was a numbness and irregular action in the muscles of the fore-arms and hands; he was incapable of feeling any minute objects, and would often let things fall from his hands. His constitution was much impaired by severe duty in a hot climate; and, after having gone through numerous trials of blisters and other remedies, under the direction of regular practitioners and empirics, he placed himself under my care. I commenced a course of electricity with him, but his strength and health were rapidly failing him, and in about three weeks he died. On examination, I found his liver and spleen considerably enlarged, and altered from their healthy structure. The lungs were tuberculated, and the pleura was pretty generally adherent to the parietes of the chest. As I was most anxious to ascertain the state of the vertebræ and medulla spinalis, I did not pay much attention to these appearances; and, having cleared away the viscera, proceeded to make an accurate inspection of the vertebral column, and no disease being apparent externally, I removed all the bodies of the vertebræ by sawing through the rings between the articular processes. By this plan, I obtained a very complete and satisfactory view of the spinal marrow, but still no sufficient cause for the symptoms presented itself; and I was left to conjecture respecting the nature of the malady. On examining the head, however, the source of the mischief became apparent. The cellular structure of the pia mater was loaded with fluid, and the tunica arachnoidea was of a milky colour and thickened, especially at the base of the brain, where it was firmly connected with the *pia mater*, forming together a tough dense membrane. I am aware that to many this may appear an insufficient cause for the violence and extent of the symptoms, but, having paid considerable attention to the morbid anatomy of the brain, and well knowing the infinite variety of symptoms produced in different individuals by the same morbid alteration of structure, I am satisfied in my own mind that all the phenomena were referable to this diseased state of the membranes. I do not, however, rest my belief on the evidence afforded by this single case, as I have seen a similar appearance in two other brains; and in both cases, although there was no absolute loss of locomotion, sensation was much impaired,—the temperature of the limbs was sensibly diminished,—and the patients tormented with involuntary action of the muscles, or cramps every night, and frequently during the day; all which symptoms I conceive to have depended on undue nervous influence. I abstain from relating these cases more fully, being unwilling to swell these pages.

In addition to the evidence I have already adduced in support of my opinion that the third case related by Mr Baynton is not a legitimate instance of diseased spine, I must add one more, which is at the present time under my care.

M. Holloway, aged three years and a half, was sent to me by an eminent physician, with a request to institute a drain in the back, for the cure of a diseased spine, which he supposed to be the cause of the loss of power in the lower part of the body. On examination, I found the whole column arched as in the former cases; one part, however, was rather more prominent than any other, at the lower part of the dorsal vertebra, and here the disease was supposed to exist. On making farther inquiries respecting the child, I found that it had never spoken, and had *never* enjoyed more power over its limbs than at the present moment. The head was evidently much larger than natural; and the fontanelle was remarkably open. The pupils were much dilated, and the bowels very torpid; indeed there was every mark of hydrocephalus, to which I was induced to refer the loss of power and all the other symptoms. In compliance, however, with the wishes of the physician and the parents, I made a seton about two inches long on either side of the most prominent part of the spine, taking care, at the same time, to express my own opinion of the case, and not auguring much good from the remedy. The setons were kept discharging for three months, and tincture of bark, with small doses of oxymuriate of mercury, were given internally. The child was kept constantly in a horizontal posture, either lying on a hard mattrass or a carpet. This was strictly enjoined, in hopes of recovering the form of the chest, which had been so much compressed on one side as almost to arrest the function of the lung. This arose from the pressure of the arm in nursing him, at a time when he was so perfectly helpless as to be incapable of supporting his head; and the whole weight of his body, unassisted by any muscular exertion, resting on the arm, had produced this effect. When the child was lying on any firm horizontal plane, the spine of itself became perfectly straight, which convinces me that in this, as in the other cases, the gradual bent position of the body depended on a want of muscular power; to which circumstance, or rather an irregular muscular action, I think most cases of lateral curve are to be referred. If any doubt remains respecting the capability of such a cause in producing such an effect, it is very easy to shew the power which the muscles exert in strengthening the joints, by dividing the tendons passing over any joint, when it will immediately become quite pliant and flexible. The same phenomenon occurs in acute rheumatism, where the muscles are no longer under the control of the will, and the joints are quite loose and powerless. The same is observed during sleep and after death. I have been induced to dwell a little on the influence which the muscles exert in maintaining the erect posture of the body, because I conceive that it is a new view of the subject, and likely to throw much light on many cases of supposed disease, which have been rather aggravated than alleviated by drains, and have afterwards been relieved by rest in a horizontal posture, which is unquestionably the best and most appropriate mode of treating such cases. The test of the truth of my position is simple and perfectly satisfactory. If a patient with a curved spine, on being placed in a horizontal posture, becomes perfectly straight, without causing any pain, or any violence being used, I do maintain that such curve does not depend on a diseased state of the bodies of the vertebræ, but on a want of muscular power. It is time now that I should proceed with Mr Baynton's cases.

Case the 4th.—Sometime in 1809, Mr B. was desired to see a young lady, who had for some time had an obscure complaint. A great degree of fever was constantly present; the bowels were very irregular, and she was much debilitated. After in vain endeavouring to discover the cause of these symptoms, it was determined to examine the spine. The cause of the mischief was immediately discovered. Five of the vertebræ were found in a diseased protruded state, and so *exquisitely tender, as to render pressure almost insupportable.* A crib, &c. were ordered, and the lady placed thereon, without any elevation of the head. Appropriate medicines were ordered, and, in two or three weeks, the amendment in her general health was astonishing. The recovery proceeded gradually without interruption or abatement, till a perfect cure was accomplished. Undeviating rest was persisted in till the expiration of the eleventh month, when this young lady arose in as straight and as perfect a state as possible. I hope that I shall not be considered obstinately sceptical if I declare that I much doubt if this was a case of diseased spine, certainly not of that disease described by Mr Pott, nor one in which any surgeon would have been warranted in using issues. It appears "that five of the vertebræ" were protruded; which of them were affected he does not mention, nor does he say *when* the back became straight. On these two important questions we are left completely in the dark. Some light, however, is thrown on the subject by the last paragraph. "She rose in as straight and *as perfect a state as possible.*" I think I may venture to say that this could not have been, if the bodies of the vertebræ had been diseased, and the curvature had been caused by their giving way."

Case the 5th is undoubtedly a case of diseased spine, which commenced in a boy aged nine years. Issues were made, and kept open for near twelve months, during the whole of which time great attention was paid to his surgeon's direction, to recline as much as possible. At the end of the time it was observed by the patient's mother that the disease had increased, and that one or two of the vertebræ projected a little; which induced the lady to call in another surgeon, who constructed an apparatus to remove the pressure, the use of which was steadily persevered in for five years and a half; at the end of which time Mr Baynton was called in. It then appeared that eleven of the vertebræ had protruded, so as to form a very large unsightly curve.* *A considerable mass of bony matter had also been deposited on each side of this curve;* and I was informed that it was absolutely necessary for him to relieve the diseased parts from the effect of pressure, by the constant daily use of the machine, whenever he attempted to sit, stand, or walk. Mr Baynton thought that the case would not get worse, and recommended a perseverance in the same treatment. In less than a year he was again brought to Mr Baynton, who found that he was still worse. The soft parts had by this time become thickened and enlarged in so great a degree as to have caused an addition to the tumour equal to its bulk when before examined. *The inflamed appearance and tenderness of the soft parts, as well as the exquisite sensibility which the bony mass had now acquired,* clearly showed that the patient's life would be endangered by any further omission of the advantages of absolute rest. A proper crib and mattrass were, therefore, immediately procured, and the patient laid upon *it without any pillow.* Rapid amendment followed, and at the termination of the fifteenth month he was allowed to rise. *His confinement (he adds) was not irksome, or productive of complainings. A previous union of the bones prevented the removal of the curve. This dreadful disease, which had existed between seven and eight years, was cured in fifteen months.*

This case is highly interesting, and requires particular attention. It was a case of disease in the spine, either commencing in the bones or intervertebral substance. It was not, however, accompanied with its usual symptom of want of muscular power in the extremities. Issues were applied, and kept open for a twelvemonth, when the mother *discovered* that one or two of the vertebræ projected a little; in consequence of which she placed him under another surgeon, and the drains were discontinued. It must be observed here, that, although the lady did not discover the protrusion earlier, it is more than probable that it existed at the time the issues were inserted, or else what could have guided the judgment of the practitioner in considering it to be a disease in the spine, particularly when one of the most marked symptoms was absent;—and what induced him to fix on that exact spot for the application of the issues, if no protrusion existed to indicate the seat of the disease? The proof of the disease having increased under the use of issues is obviously deficient, without the testimony of the first surgeon, whose name is unfortunately suppressed. After the issues were healed up, and instruments used for supporting the trunk, the case got progressively worse during the space of six years and a half, at the end of which time he was in the deplorable state described above; and no doubt had the instruments been persevered in, death would have soon followed. No one I think can doubt that the sufferings of this patient, and the continuance of the disease, were prolonged by the improper means which were resorted to, and certainly the poor boy has cause to rue the day when his mother made the discovery of the protrusion; for, had issues been persevered in, and no instrument been used, I do not hesitate to say that he would have recovered infinitely sooner, and with less deformity. This forms a most striking picture of the impertinent interference of art in counteracting the effects of nature. To prove this, it is only necessary to consider what are the natural terminations of the complaint, and what the rational curative intentions should be. When the disease has proceeded so far as to cause a complete absorption of the bodies of some of the vertebræ and intervertebral substance, it is impossible that such parts should ever be restored, and all that we can look for is an anchylosis between those bones which are brought nearly in contact by the removal of the intermediate parts. This healthy process can never take place, until either the diseased action ceases spontaneously, or is arrested by art. If the powers of the constitution are vigorous, the deposit of new bone may possibly be considerable, and fill up more or less vacancy; but this I believe rarely happens to any extent. It is obvious, then, that any attempts which are made to separate the surfaces, must retard and prevent the cure. The curative intention of such a case would clearly be, to keep the patient perfectly quiet, to amend his constitution by suitable remedies, and to check its further progress by counter-irritation. In the present case it is clearly manifested, that, when Mr Baynton first saw him, the disease had stopped, and the "debility of vessels" which Mr Baynton speaks of, no longer existed, as a considerable "*bony mass was deposited on each side of the affected vertebræ*," but, by the improper use of mechanism to elevate and to separate the bones, anchylosis was prevented. When he afterwards saw him, after the interval of a twelvemonth, "*bony union had taken place, which prevented the removal of the curve*." Is it probable that, if such union had not taken place, that, by an undeviating horizontal posture, the space occupied by eleven bodies of vertebræ would have been supplied, and deformity prevented? not to mention the alteration in the chest, and the gradual adaptation of the surrounding parts to their more circumscribed bounds,—which accompanies the deformity of the back,—all which must be removed in order to restore the patient to his perfect stature. From a careful review of every circumstance of this case, I am clearly of opinion that the diseased action had ceased when he was placed under Mr Baynton's hands, and that the perfect state of rest in which he was kept favoured the anchylosis, and was the best treatment that could be adopted; but I cannot admit that rest had any share in stopping the disease. Yet, to this only he would seem to refer the happy termination of the case, and would imply that rest is all-powerful in such formidable diseases.

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* It does not appear that there was any paralytic affection in this case, where eleven vertebræ were diseased, yet Mr Baynton talks "of the compression of the medulla."

One more observation with regard to this case, and I will then proceed in my examination. In one part of his relation he speaks of the "tenderness of the soft parts, and the exquisite sensibility of the bony mass;" yet he says "his confinement was not irksome, or productive of complainings." Precisely the same observations occur in the fourth case. How these contradictions can be reconciled, I know not; or how a patient's suffering exquisite pain from pressure, could bear resting with the whole weight of his trunk on the inflamed and painful part, and yet not consider his confinement irksome, I confess to be above my comprehension:—not to mention the dreadful inconvenience in swallowing, with the head drawn back to such an extent, which it must be to rest upon the same level with the rest of the body, in a person with so great a protrusion of the dorsal vertebræ as has been described.

The next case, No. 6, I shall not have occasion to dwell upon, as the subject of it, a Mr Emerson, died before Mr Baynton's plan was put in execution, and consequently no conclusion of its efficacy can be drawn.

Cases 7th and 8th are both instances of common lateral curve, which never takes place from disease in the bodies of the vertebræ; in which neither Mr Pott, nor any surgeon conversant with the principles of his profession, would have ever dreamed of applying issues.

Case 9th.—Miss ——, aged 15, was observed to become listless, to lose her health, and to be incapable of walking properly, about the end of July 1811. On examining her spine, I discovered that five of the dorsal vertebræ were in a diseased and considerably protruded state; and that *those bones, as well as the contiguous soft parts, were incapable of bearing the slightest pressure, without the production of a considerable degree of pain.* She was subjected to the resting process, and in seven months the tenderness and *protrusion of the vertebræ were entirely removed*; the limbs had recovered their strength and motion, and it was apparent to every person that she was in a state of perfect health. It was also ascertained that she had acquired several inches in stature." In the relation of this case, it is a pity that symptoms are not described with more minuteness. It appears "that she was incapable of walking properly," but it is not mentioned whether this was accompanied by the peculiar characters of diseased spine laid down by Mr Pott. Mere debility, and the stooping of the body, would prevent a person walking properly, but not produce the symptoms above named. The perfect restoration of the vertebræ, however, would induce me to be sceptical respecting the nature of the disease, which certainly does not at all correspond with my ideas of destruction of the bodies of the vertebræ. The diseases of the spine vary as much as diseases of other parts, and to some issues are applicable, to others not; but I do not think that the present case can at all invalidate Mr Pott's practice, as I do not consider it at all analogous to his description of the disease.

Case 10th.—Master W. Castle, when about a year old, was observed to have lost his health, and it was discovered that he could not move his legs as well as he had been accustomed to do. On examining the spine, I discovered that four of the dorsal vertebæ were in a diseased and protruded state, and that great pain was occasioned by moderate compression, either of the bones or contiguous soft parts. "Absolute rest and the muriate of lime were resorted to. At the end of the tenth month he was allowed to roll and play on a carpet; in a short time after he got on his feet, without assistance, and has continued in perfect health from that time to the present. The curve is entirely removed."

The treatment of this case was certainly highly judicious; but surely this can never be brought forward as a case of scrofulous spine. Any one at all conversant with the treatment of infants, knows how much their power of using their limbs depends on dentition, and how often, to use the nurses' expression, a child is taken off its legs during this period. With respect to the curvative of the vertebræ, it is well known how very pliant the spine of children is, and I have seen a great number of weakly children, in whom this growing out of the back has been apparent, and created much alarm to both nurse and parents. When I first met with such cases, I fell into the same error with Mr Baynton, and supposed them to be cases of diseased spine; but having since had greater opportunity of seeing children's diseases, I am persuaded that this is merely the effect of debility, and that it will always be removed by keeping the child off his legs, and giving such medicines as are calculated to strengthen the constitution generally. Mr Baynton says, the "curve is entirely removed." When this alteration took place he does not mention; but I suspect that the moment the child was placed in a horizontal posture, it ceased to exist.

Case the 11th is drawn up by the young lady herself in the following words: "June 1811. Occasionally a slight pain in the left leg; sometimes a little swelling pain, increased by degrees until December following, when both legs were almost constantly in pain; not able to take much exercise; frequent pains in the head, chest, and side. February 1812. Sometimes a pain in the back, which, from the beginning of March, became constant. From this time I was unable to lift up my legs, or move them without great inconvenience. My strength decreased every day, and the pain in my chest, &c. became much more frequent, and violent, which continued until May following, when I lay down, and in *less than a week* every unpleasant feeling was removed. After a confinement of seven months, I got up perfectly strong and well. Aged, when I lay down, fourteen years five months."

On what possible ground can this statement be construed into a disease of the spine, and brought forward to invalidate the efficacy of issues? It must, I think, be apparent to every one that these symptoms might, with great probability, depend on the period of life of the young lady. The speedy relief, after one week's rest, might take place on commencing menstruation, which would fully account for the previous swelling of the legs, pain in head, back, &c. This case I conceive requires no further comment.

Case the 12th is drawn up at great length by the patient's mother, in September. It appears to be a genuine case of diseased spine, for which issues were ordered by Mr Keate, about the end of January 1811, which were badly managed, and not properly kept open; and the patient being suffered to walk about, the symptoms increased, the lower extremities became useless, and the bladder incapable of expelling its contents. In July the issues were healed up, and she was placed under Mr Baynton's care, and subjected to rest. "At this time she had so entirely lost the use of her legs, that when she stood she had not the power of moving them. *They were not, however, without sensation, or at this time were they ever cold, but were subject to involuntary twitchings or drawing up many times in a day or night.*"

From this time she gradually grew better, the twitchings left her, and she began to recover power over her limbs and bladder. She continued lying down for a year and a fortnight, after which time she was permitted to sit up.

"In this lady's case," Mr Baynton adds, "six of the dorsal vertebræ were in a diseased, protruded state. At the time of recovery, the curve was considerably lessened, but not entirely removed. Its complete removal had been prevented by the previous union of two of the vertebræ."

From the history of this case there can be no doubt of its nature, or of the propriety of using issues, combined with rest. The aggravation of the symptoms, under their application, may readily be accounted for by their not being properly kept open, and the patient being permitted to use exercise at a time when the bodies of the vertebræ were in a diseased state, and incapable of bearing pressure. Yet, under all these disadvantages, it is obvious, from Mr Baynton's own statement, that they had been of material service in arresting the disease, as it appears that "a previous union of two of the vertebræ" had taken place when she was first sent to him. My opinion on this head is further confirmed by the statement of the progress of the case, at the time when she was placed under his care. She had lost the coldness in her limbs, and was tormented with involuntary twitchings of the muscles; which last symptom I have always hailed as a most favourable omen, having repeatedly observed that this involuntary action of the muscles preceded a return of voluntary motion, and is the first proof of the complaint being arrested. In the present case, it is probable that if rest had been combined with the drains, the cure might have been accelerated, but I cannot admit the conclusion that rest alone effected the restoration to health.

Mr Baynton, in this, and other cases, speaks more definitely respecting the exact vertebræ that were diseased, than from the nature of the disease he can possibly be warranted in. When the body of a single vertebra has been absorbed, and the contiguous ones approximated, it will produce deformity, and lead a person to suppose the disease to comprehend a much greater extent. After a consolidation has taken place, and the patient is restored to health and strength, he will hold himself much more erect, and the curve will be more defined; but we are not to conclude from this that all the vertebræ, comprehended within the original curve, have been diseased.

Another circumstance to be remarked in Mr Baynton's statements of the extent of disease in several cases, is that it has been situated, in most instances, in the dorsal region, where there is naturally a considerable excurvation in the healthy state, giving the vertebral column the form of an italic *S*. This natural projection, when increased by debility and want of muscular action, is a very common source of error, and should at all times be taken into account, in estimating the extent of disease.

Case 30th.—The first part of this case is drawn up by the father of the child, the latter part by Mr Baynton. The following are the principal facts. Master Wetherell, when three years old, became sickly, and complained of pains in his back and legs. These symptoms continued for two years, when a projection of the vertebræ was perceived. In November 1811, issues were applied, and the child *confined to his couch in a reclined position.* The issues were kept open until the following April, when they were healed, from the father conceiving that the child was worse in consequence of his increased difficulty in walking, which he could only effect by putting his hands on his knees, and his stooping having greatly increased. In September he was placed under Mr Baynton, who gives the following account. "Nearly all the dorsal vertebræ were in a diseased state. They had yielded so much to the pressure of the parts above as to have formed a curve nine or ten inches long, of a pyramidal shape, that projected from its base to its point four or five inches; a considerable quantity of bony matter had been deposited along each side of the projecting vertebræ. The contiguous soft parts were thickened, and very tender. His ribs had lost their arched form, and were so flattened as to have occasioned the breast-bone to project some inches above the chin, when he was placed on his back. His belly is remarkably large and tense; his appearance was that of an emaciated and almost dying child." He was subjected to the treatment recommended by Mr Baynton; improvement was soon visible; "*in a few weeks* his health was perfectly restored." After nine months he was allowed to return home, at which time the curve was entirely removed, except in the middle part, *where a previous junction of two of the vertebræ had rendered it impossible.* His ribs had recovered their arched form; his breast had become as flat and natural as the breast of any child."

I have so far anticipated any critique on this case by the observations I have already made, that there is but little room for further remark. Mr Baynton appears to have been singularly fortunate in the exact period when this, like some other cases, fell under his treatment. As it appears from his own statement, that bone was deposited on each side of the projection, and an union between the vertebræ had been effected;—as I conceive that this ossific union is the great desideratum, and that it never can take place until the diseased action has ceased, —I must contend that this is not a legitimate instance of a cure of diseased vertebræ effected by rest alone.

These are the only cases which Mr Baynton has published in support of the treatment of diseased spines by undeviating rest; yet, on this feeble and imperfect evidence, the reviewer has thought fit to explode the practice which has been sanctioned and approved by most of the practitioners in Europe, and has ventured to cast unwarranted imputations on the character and reputation of one of the most upright and enlightened men that ever contributed to the improvement of the profession. It is unquestionably the province of a reviewer to be slow and cautious in belief, and to detect and point out errors; but there is a wide difference between a clear and manifest exposition of faults, and a bare unqualified assertion, "That it is impossible not to distrust the testimonials, strong as these of course are, in favour of the practice recommended by Pott, Earle, and other distinguished men." From how feeble a source the writer's incredulity has arisen I trust that I have established in the preceding pages.

It was my intention to have subjoined some cases and observations on the various diseases of the vertebræ and medulla spinalis, but, fearing that I should exceed the limits of a journal, I shall defer them until some future time.

10, *Berners Street, August* 29, 1814.

IV.

On the Relation subsisting between the Time of the Day, and various Functions of the Human Body; and on the Manner in which the Pulsations of the Heart and Arteries are affected by Muscular Exertion. By ROBERT KNOX, M. D. Edinburgh.

To the Editor of the Edinburgh Medical and Surgical Journal.

SIR,—I have ventured to submit to you for your approbation some observations and experiments on a subject, which, for some time, has occupied a considerable share of my attention. The matter itself will, perhaps, at first sight, appear to many rather curious, than interesting or useful. But it ought to be remembered that observations, in themselves apparently of little direct importance, have, in the progress of time, been found much to benefit science.

I may here briefly mention the circumstances which first induced me to investigate opinions, to the correctness of which so many had unlimitedly subscribed. Annoyed like others with the interminable disputes concerning the *stimulant* and *sedative* powers of foxglove, I resolved to make a few experiments on that celebrated drug. A variety of perplexing circumstances soon convinced me of the necessity there was, correctly to ascertain the various conditions of the healthy pulse, particularly as regarding muscular exertion, diet, &c. This knowledge, however, I found was not to be attained in a short time, nor without considerable labour. Contrary to my expectations, that which I deemed of minor importance, or merely preparatory to other researches, became a valuable subject of inquiry.

Whilst conducting the experiments about to be related, I have often almost despaired of reducing to general laws the endless variety which the functions of the animal economy present to every observer. But remarking, amidst this seemingly inextricable confusion, a certain degree of regularity, I was induced and encouraged to prosecute the inquiry, convinced that we ought never to disregard those signs of order which nature manifests, whether they regard animate or inanimate beings.

It is almost needless for me to observe, that every precaution has been taken to avoid error. If, throughout the essay, this desirable object be attained, it is principally attributable to the assistance of my friend Dr James Thomson, continued with unremitting zeal and friendship, during a course of experiments both tedious and delicate. [*]

SECTION I.

1*st*, When we recollect that it is during the course of the day that all those circumstances, which tend to accelerate the pulse, are generally combined and present, the proposition that the morning pulse of a person enjoying health is quicker than the evening one, must seem almost a paradox; at all events, so hostile to what appears probable or plausible, that the clearest evidence of its truth must be brought forward before we could expect to gain a single proselyte.

In detailing the results of my observations and experiments on this subject, a certain degree of license is necessarily made use of, which, however, does not at all affect the conclusions I have drawn. In these experiments, it is not proposed to submit the functions of the body to rigorous calculation. In physiology, as well as in medicine, a near approximation to the truth is the only rational end we can possibly propose. Numbers are used, both because they render our ideas more precise, and cause to be distinctly perceived the limits to which we should wish to extend these doctrines. The variation in different individuals must necessarily be immense;—for that the candid will make proper allowance.

Dr Cullen, (whose name is not merely celebrated, but venerated in medical science,) talked of a natural acceleration of the pulse, which, according to him, happens twice a-day, and resembles, in a distant manner, a febrile paroxysm. Now, amidst many hundred experiments, performed under a great variety of circumstances, I could never perceive any such phenomenon. The time of the day at which the first spontaneous acceleration is supposed to happen, is noon. My own pulse never shewed any symptoms of such acceleration, but, on the contrary, always diminished in velocity. Thus,

Ex. 11, A. M........Pulse 72
12, noon,.............. 71
1, P. M..............65
Half past 1, P. M......64

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[*] The subject of the experiments is about 22 years of age, of a moderate height, and somewhat muscular; his constitution may be called irritable,—by which is meant only, that it is easily excited by stimulants of almost every kind. He has not laboured under any serious indisposition for a great number of years, nor is he conscious of any hereditary or acquired tendency to disease in any organ.

The other paroxysm, according to the same author, occurs in the afternoon, and that, too, totally independent of any excitement by food, &c. This opinion is equally erroneous with the former.

Ex.	3, P. M. Pulse	68
	4,	66
	5,	64
	6,	62

Ex.	Half past 3, P. M. Pulse	67
	Half past 4, P. M	67
	5, P. M	64
	Half past 5,	63
	6, P. M	63
	Half past *6*, Pulse	62
	7, P. M.	62
	Half past 7,	60
	8, P. M	58

These experiments are in direct opposition to the statements of Cullen ; yet, whoever carefully peruses that author's works, more anxious to discover truth, than, by partial views and misrepresentations, to detect contradictions, will be convinced that Cullen's notions are partly just, and partly erroneous ;—that delicate and accurate observation is blended throughout with supposition, assertion, and hypothesis.

2d, This quickness of the arterial pulsations, observable in the morning, is at present very generally disbelieved ; yet nothing can be more easy than to ascertain the fact experimentally. Perhaps the following instances, taken at random from a great number of others, may go far to settle the point. In the following Table the pulse was reckoned in the sitting posture, (this is always to be understood, unless the contrary be expressed) before breakfast, and a short time after rising from bed ; and, in the evening, after a light supper, and generally some spirituous liquor, taken, however, in small quantity, and much diluted. The power of alcohol in raising the pulse is well known. The period is the summer time.

TABLE.

About 9, A. M.		About 12, P. M.	
68	68	63	63
65	74	70	70
68	70	62	66
68	69	61	62
67	65	59	63
62	70	63	72
70	71	62	72
70	68	61	72
64	76	66	62
The average result of these is 68.5.		The average is 64.38.	

To render the result of this table the more striking, it is necessary for me to point out a few circumstances, to which considerable attention ought to be paid. In the morning, the pulse was reckoned immediately almost on rising from bed, and always before food of any kind was taken. Every circumstance, therefore, conspired to reduce the pulse. On the other hand, the evening pulse was reckoned after the many exertions undergone during a long day,—after exercise of mind and body,—after food and drink; the pulse is, notwithstanding, considerably less frequent than in the morning, when every thing favourable to a rapid state of the circulation was carefully guarded against. But the pulse is not only in general quicker in the morning than in the evening; it is also more excitable; *i. e.* the same quantity of food or drink, the same degree of exercise, shall be found to exalt the pulse more in the morning than at any other period of the day or night; and the capability of being excited may, generally speaking, be said to diminish from a very early hour until the same hour next morning. The *data* on which this assertion rests are pretty numerous, though not so complete as I could have wished them to be. To accomplish this, would have required a complete change of my mode of life, which I found at the time impracticable.

TABLE

Shewing the different states of the pulse, as reckoned at three different periods of the day, viz. after breakfast, dinner, and supper. The experiment was made during summer.

After breakfast, always before 10, A. M.	After dinner, before 5, P. M.	Evening, between 10 and 12, P. M. generally near 12.
Pulse 66	Pulse 68	Pulse 63
68	71	70
69	73	62
66	69	61
69	71	59
69	74	62
70	76	65
64	66	63
80	80	68
73	80	70
75	76	76
80	76	68
74	84	60
74	72	63
78	72	59
75	72	62
76	80	66
73	76	62
Average 72.	Average 74.22, &c.	Average 64.388, &c.

The conclusions to be drawn from this table are obvious. To render them still more so, I may obsesve, that the breakfast is generally a moderate one, consisting of coffee, bread, and eggs; the dinner always of animal food principally, and a small quantity of vegetables; to which was often added a little spirits or porter; and, notwithstanding the vast difference between the stimulating powers of these two meals, the morning pulse is inferior to the one after dinner by only two beats; the difference would have been ten or fifteen, had the case been reversed, at least I am induced to think so from some experiments which I afterwards instituted directly to ascertain the point. In the evening the pulse was reckoned after supper, light, indeed, but certainly equal to the breakfast in stimulant power, and in some measure more so, because spirituous liquors very generally supplied the place of coffee. Notwithstanding this, the evening pulse is found to be, on an average, nearly eight beats lower than the morning one; a difference by no means inconsiderable, and sufficiently warranting the conclusions I have drawn from it.

The following table was drawn up, in order to ascertain the effects which moderate exercise in the morning would produce on the pulse, reckoned, however, in such a manner as not to be immediately affected by it.

TABLE.

A walk of three miles before breakfast. Pulse after breakfast.	Pulse after dinner, as usual.	Pulse after supper, as usual.
74	76	62
81	78	62
80	75	62
83	78	64
80	75	66
78	74	64
Average 79.33.	Average 76.	Average 63.3, &c.

3. My experiments have not yet enabled me to ascertain, with precision, the hour when the pulse begins not only to be actually more rapid, but also to acquire a greater capability for action, manifested by the exhibition of any stimulant. This, with myself, perhaps takes place about 3 P. M. (A. M.?); but there is every reason to believe that the time varies with the individual, the season, climate, and perhaps, though this for many reasons is improbable, with the mode of life.

4. This increased capability for action, occurring regularly in the morning, is even of greater importance than the *actual state*

of the circulation; the former may be made the subject of very delicate experiments; the latter we know to be exposed to a thousand variations, from causes which have been in part developed. While experimenting on this subject, there occurred to me a case, altogether of so interesting a nature, that I cannot refrain from giving it in detail. It affords a remarkable proof of the truth of the doctrines brought forward in this essay; at the same time, independent of this consideration, from the rarity of its occurrence, it appears to be worthy of record.

M. C. aged 4.—The account we received from the parents of this child was, that she continued in good health until about six months after birth, when a blueness of the surface was remarked, particularly on any exertion, and accompanied with perpetual difficulty of breathing. In this state she continued to grow, and became remarkably tall of her age. She was observed, however, to get daily worse; that is, the paroxysms of threatened suffocation became more frequent, during which the whole surface of her body appeared almost black. A strong beating was constantly to be felt in the region of the heart, and she was sometimes convulsed; muscular debility great. At her death she measured three feet four inches. The body continued warm for five hours after death.

Dissection.

1. *External appearances.* The countenance, feet, and body, were not all discoloured; the arms and fingers retained their usual dark colour.

2. *Thorax.* The lungs seemed sound, though dark-coloured; heart perhaps larger than usual; *foramen ovale* pervious, and pretty large. The *aorta* arose from both ventricles in such a manner, that the ventricles communicated with each other, and with the cavity of the vessel, which was much enlarged; its *vasa vasorum* distended with red blood. From the right ventricle there arose a pulmonary artery, very small in its diameter, (perhaps the size of a small goose quill) and feeble in its coats; it possessed all the characters of the artery, such as its valves and division into two branches. Not the smallest vestige of a *ductus arteriosus*; it seemed never to have existed. The passage from the left ventricle into the artery, common to both, was more direct than that from the right. Every where the arteries were full of dark blood, not coagulated.

3. The abdominal viscera were of a very dark colour. The head was not examined.

Before leaving the house, the father of the child related to us a circumstance to which we all paid particular attention. The child, he informed us, had always been very fretful and peevish, (perhaps from indulgence,) and apt to have frightful paroxysms when spoken to harshly; but these happened much more frequently in the morning; indeed he observed that she was always ill in the early part of the day; so much so, that the smallest exertion, an angry word, or even a cup of tea, would instantly bring on a paroxysm, each time threatening destruction. This tendency to these fits decreased from morning, till about 5, P. M. from which time she generally continued well till bed-time. The father detailed this with great minuteness, and seemed to wish for an explanation.

It will be easy to anticipate to what cause I would ascribe this increase of disorder in the circulation, during the forenoon. So disproportionate was the pulmonary artery to the aorta, it was with difficulty that life could be carried on; but, in the morning, when the susceptibility of the arterial system was greater, the inequality must also have been increased to such a degree as almost to have destroyed life. I may also remark that she died about 9, A. M.

5. To be convinced that an opinion, very different from that now brought forward, is the one more generally received, we have only to turn to the writings of a few celebrated physiologists. Thus, for example, in the "Physiologie Positive" of Fodéré, a work of merit, we find the following remarkable passage: "Relativement à la difference du jour et de la nuit, on remarque que le pouls de l'homme adulte bat de 60 à 65 fois par minute, au commencement du jour, et qu'il va continuellement en augmentant jusqu'à battre 80 fois dans le même temps, chez les plus excitables, sur la fin de la journée; dans la nuit, les pulsations diminuent de nouveau, jusqu'au matin, où elles se trouvent revenues insensiblement au nombre de 60 à 65." * In order to have an approximation to the truth, we have only to reverse the above statement. If we regard as accidental circumstances the accelerations of the pulse, occasioned at various times by our diet, and that, too, according to the caprice of the individual, the pulse shall be found gradually to diminish in velocity from an early hour until midnight, and generally later. This difference I have found to be great in winter, but less, though still existing, in summer, as the above experiments, per-

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* Essai de Physiologie Positive, Tom. I. p. 190.

formed during that period, sufficiently prove; and from this I am inclined to believe, that heat has no inconsiderable share in the production of the phenomenon, though totally inadequate solely to produce it. This gradual excitation in the number of the pulsations Fodéré conceives to be owing to the combined effects of exercise, of the action of the internal and external senses, of light, heat, purer air, &c. Violent exercise towards evening will certainly produce a considerable excitation of the pulse, but less than what takes place in the morning, in consequence of the same degree of exercise; neither will any degree of heat, nor quantity of food, raise the pulse in the evening to the height at which it is in the morning, almost without any such excitants, provided they be not used in an immoderate degree.* Thus, after fasting till 8 P. M. I found my pulse to be 58; at 9 P. M. after eating a moderate dinner, pulse 58.

The common mode of experimenting with unequal quantities of food, taken at various hours of the day and night, was evidently insufficient to establish the principle in its full extent. I endeavoured, accordingly, to submit to a particular regimen, and examine the state of the pulse under the influence of a diet always similar at every hour of the day. This decisive experiment could not be pushed any length; so difficult a matter did I find it to break through, at once, those habits which a great number of years had firmly established.

6. "All these things (continues Fodéré) singularly facilitate the return of the venous blood towards the heart; and this is one of the causes of the evening paroxysm which takes place in all fevers." But, having denied that any such gradual increase of the pulse exists, we need scarcely stop to say, that this cannot be one of the causes of the evening paroxysms in fever.

This opinion appears to be borrowed from the celebrated Cullen. That author, however, increased the facility of explanation, by describing two exacerbations or augmentations of the pulse; one about mid-day, the other towards evening. This explains, in his opinion, the occurrence of the double paroxysm in hectic fever.† The whole is a pure *hypothesis*, which seems partly to have arisen from supposing that the pulse was governed by the same laws in health and disease. So far as my observations go, the order of nature in disease is entirely reversed, and the observations of Cullen, Fodéré, and others, are applicable only to the unhealthy. That this acceleration of the pulse (which is really an aberration from the regular laws of nature) actually happens in fever, the testimony of Cullen, generally so accurate in the observance of disease, will sufficiently establish; and that this universally takes place in phthisis, we have, unfortunately, too many opportunities to verify.* It was from the sinking of the pulse towards evening that I ventured, independent of other circumstances, to prognosticate favourably in the case of a child labouring under typhus fever. It was this which induced me to hope that some of the functions had begun to resume their natural order, and that recovery was a probable event. Nor was I deceived; by proper attention the patient from that day rapidly amended.

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* In experimenting on this point, care must be taken that the acceleration of the pulse, occasioned by indigestion, be not confounded with the natural healthy pulse, produced by the simple stimulation of the food.

† First Lines of the Practice of Physic, Vol. I. c. 862.

7. The physiologists who invented the various diurnal accelerations of the pulse, one, two, or more, just as suited their fancy or necessity, will perhaps find little difficulty in explaining the gradual diminution of the pulse, should the phenomenon appear to them to be correctly stated. With me, I confess, the cause is yet excessively obscure, because I have not found it connected with any circumstance, to the influence of which I might ascribe it. Experiment showed me that no previous exhaustion from labour, excess in food, drink, &c. rendered the diurnal diminution of the pulsations more evident than usual; the want of food, perhaps, hastens and increases it, but certainly does not prevent or retard it. Neither is sleep the cause of the restoration of the susceptibilities and velocity of the morning pulse, since the negation of that does not at all destroy the *excitability* † of the sanguiferous system, as manifested by the application of the accustomed stimulants.

August 30th 1813, the day being moderately warm, I walked, between 1 and 11 P. M. a distance of nearly 40 miles. Not having much appetite, retired to rest about 1 A. M. after drinking a little coffee, but slept none, perhaps owing to over-fatigue. Next morning (31st) about 7, my pulse was 80 and rather feeble; after breakfast, before which I took a small glassful of spirits, my pulse rose to 104. I was not feverish, and performed a journey that day of 27 miles, at a tolerable pace.

To what, then, are we to attribute this daily diminution of velocity in the functions of the sanguiferous system? Its existence as a law of the sanguiferous system, has been demonstrated experimentally; and I shall endeavour to shew, that a similar revolution daily takes place in several other functions of the human frame. To this conclusion Dr Cullen arrived, merely from observing the daily returns of sleep and watching, of appetites and excretions, and the changes which regularly occur in the state of the pulse. Of these changes he had, however, no clear ideas. When he proceeds to combine this diurnal revolution with the phenomena of fever, he unquestionably offers a conjecture, extremely probable, ingenious, and perhaps original; but unfortunately there results from this fine idea, nothing but conjecture and hypothesis, attributable principally to an incorrectness in the observance of the phenomena.

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* Beddoes' Essay on Consumption, p. 252.

† I use this word merely to avoid circumlocution.

8. It required few experiments to convince me, that animal food raised the pulse much more than vegetable; the excitation of the pulse by wine is still greater, and that from spirituous liquors greatest of all. By these circumstances, but more especially by diet, the regularity in the diurnal revolution in the pulse, is, as might have been anticipated, much disturbed. But it was impossible to perform these experiments without remarking, that something similar happened to various other functions. Thus, beyond all doubt, our perceptions in the early part of the day are clearer, our minds more acute, our whole intelligence more active. The functions of the stomach seem also much stronger at this time than towards evening. Feverish, restless nights, are the invariable attendants on late meals, which injure in the *ratio* of their quantity.

I have repeatedly remarked, that digestion went on more easily in the morning than in the evening. Three or four times have I been induced (sometimes compelled), whilst following some favourite sport in the country, to defer the taking food until evening; a greater or less degree of fever and restlessness, in proportion to the quantity of food taken, has uniformly followed such indulgence.

It was long with me a problem difficult of solution, why digestion should go on laboriously during the evening, when the actions of the muscular system were almost entirely suspended; more especially when I recollected, that the opinions of most, perhaps all physiologists, led us to conclude, that rest greatly favoured the digestion of our food. The objection, that during sleep, in which state the repose of the body is complete, digestion, nevertheless, is generally ill performed, seemed at first insurmountable; and it is not a little remarkable, that those physiologists who have so repeatedly stated the fact, have as constantly failed to note the objection. On observing that practical men expressly stated the necessity of rest for the right performance of the function of digestion, and experiencing

IV.

On the Relation subsisting between the Time of the Day, and various Functions of the Human Body; and on the Manner in which the Pulsations of the Heart and Arteries are affected by Muscular Exertion. By ROBERT KNOX, M. D. Edinburgh.

To the Editor of the Edinburgh Medical and Surgical Journal.

SIR,—I have ventured to submit to you for your approbation some observations and experiments on a subject, which, for some time, has occupied a considerable share of my attention. The matter itself will, perhaps, at first sight, appear to many rather curious, than interesting or useful. But it ought to be remembered that observations, in themselves apparently of little direct importance, have, in the progress of time, been found much to benefit science.

I may here briefly mention the circumstances which first induced me to investigate opinions, to the correctness of which so many had unlimitedly subscribed. Annoyed like others with the interminable disputes concerning the *stimulant* and *sedative* powers of foxglove, I resolved to make a few experiments on that celebrated drug. A variety of perplexing circumstances soon convinced me of the necessity there was, correctly to ascertain the various conditions of the healthy pulse, particularly as regarding muscular exertion, diet, &c. This knowledge, however, I found was not to be attained in a short time, nor without considerable labour. Contrary to my expectations, that which I deemed of minor importance, or merely preparatory to other researches, became a valuable subject of inquiry.

Whilst conducting the experiments about to be related, I have often almost despaired of reducing to general laws the endless variety which the functions of the animal economy present to every observer. But remarking, amidst this seemingly inextricable confusion, a certain degree of regularity, I was induced and encouraged to prosecute the inquiry, convinced that we ought never to disregard those signs of order which nature manifests, whether they regard animate or inanimate beings.

It is almost needless for me to observe, that every precaution has been taken to avoid error. If, throughout the essay, this desirable object be attained, it is principally attributable to the ass[illegible]mes Thomson, continued with unremitting zeal and friendship, during a course of experiments both tedious and delicate. *

SECTION I.

1*st*, When we recollect that it is during the course of the day that all those circumstances, which tend to accelerate the pulse, are generally combined and present, the proposition that the morning pulse of a person enjoying health is quicker than the evening one, must seem almost a paradox; at all events, so hostile to what appears probable or plausible, that the clearest evidence of its truth must be brought forward before we could expect to gain a single proselyte.

In detailing the results of my observations and experiments on this subject, a certain degree of license is necessarily made use of, which, however, does not at all affect the conclusions I have drawn. In these experiments, it is not proposed to submit the functions of the body to rigorous calculation. In physiology, as well as in medicine, a near approximation to the truth is the only rational end we can possibly propose. Numbers are used, both because they render our ideas more precise, and cause to be distinctly perceived the limits to which we should wish to extend these doctrines. The variation in different individuals must necessarily be immense;—for that the candid will make proper allowance.

Dr Cullen, (whose name is not merely celebrated, but venerated in medical science,) talked of a natural acceleration of the pulse, which, according to him, happens twice a-day, and resembles, in a distant manner, a febrile paroxysm. Now, amidst many hundred experiments, performed under a great variety of circumstances, I could never perceive any such phenomenon. The time of the day at which the first spontaneous acceleration is supposed to happen, is noon. My own pulse never shewed any symptoms of such acceleration, but, on the contrary, always diminished in velocity. Thus,

Ex.	11, A. M........	Pulse 72
	12, noon,............	71
	1, P. M..............	65
	Half past 1, P. M.......	64

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* The subject of the experiments is about 22 years of age, of a moderate height, and somewhat muscular; his constitution may be called irritable,—by which is meant only, that it is easily excited by stimulants of almost every kind. He has not laboured under any serious indisposition for a great number of years, nor is he conscious of any hereditary or acquired tendency to disease in any organ.

The other paroxysm, according to the same author, occurs in the afternoon, and that, too, totally independent of any excitement by food, &c. This opinion is equally erroneous with the former.

Ex. 3, P. M. Pulse 68
4,..............66
5,..............64
6,..............62

Ex. Half past 3, P. M. Pulse 67
Half past 4, P. M.......67
5, P. M................64
Half past 5,...........63
6, P. M................63
Half past 6,.....Pulse 62
7, P. M............... 62
Half past 7,...........60
8, P. M................58

These experiments are in direct opposition to the statements of Cullen; yet, whoever carefully peruses that author's works, more anxious to discover truth, than, by partial views and misrepresentations, to detect contradictions, will be convinced that Cullen's notions are partly just, and partly erroneous;—that delicate and accurate observation is blended throughout with supposition, assertion, and hypothesis.

2d, This quickness of the arterial pulsations, observable in the morning, is at present very generally disbelieved; yet nothing can be more easy than to ascertain the fact experimentally. Perhaps the following instances, taken at random from a great number of others, may go far to settle the point. In the following Table the pulse was reckoned in the sitting posture, (this is always to be understood, unless the contrary be expressed) before breakfast, and a short time after rising from bed; and, in the evening, after a light supper, and generally some spirituous liquor, taken, however, in small quantity, and much diluted. The power of alcohol in raising the pulse is well known. The period is the summer time.

TABLE.

About 9, A. M.		About 12, P. M.	
68	68	63	63
65	74	70	70
68	70	62	66
68	69	61	62
67	65	59	63
62	70	63	72
70	71	62	72
70	68	61	72
64	76	66	62
The average result of these is 68.5.		The average is 64.38.	

To render the result of this table the more striking, it is necessary for me to point out a few circumstances, to which considerable attention ought to be paid. In the morning, the pulse was reckoned immediately almost on rising from bed, and always before food of any kind was taken. Every circumstance, therefore, conspired to reduce the pulse. On the other hand, the evening pulse was reckoned after the many exertions undergone during a long day,—after exercise of mind and body,—after food and drink; the pulse is, notwithstanding, considerably less frequent than in the morning, when every thing favourable to a rapid state of the circulation was carefully guarded against. But the pulse is not only in general quicker in the morning than in the evening; it is also more excitable; *i. e.* the same quantity of food or drink, the same degree of exercise, shall be found to exalt the pulse more in the morning than at any other period of the day or night; and the capability of being excited may, generally speaking, be said to diminish from a very early hour until the same hour next morning. The *data* on which this assertion rests are pretty numerous, though not so complete as I could have wished them to be. To accomplish this, would have required a complete change of my mode of life, which I found at the time impracticable.

TABLE

Shewing the different states of the pulse, as reckoned at three different periods of the day, viz. after breakfast, dinner, and supper. The experiment was made during summer.

After breakfast, always before 10, A. M.	After dinner, before 5, P. M.	Evening, between 10 and 12, P. M. generally near 12.
Pulse 66	Pulse 68	Pulse 63
68	71	70
69	73	62
66	69	61
69	71	59
69	74	62
70	76	65
64	66	63
80	80	68
73	80	70
75	76	76
80	76	68
74	84	60
74	72	63
78	72	59
75	72	62
76	80	66
73	76	62
Average 72.	Average 74.22, &c.	Average 64.388, &c.

The conclusions to be drawn from this table are obvious. To render them still more so, I may obsesve, that the breakfast is generally a moderate one, consisting of coffee, bread, and eggs; the dinner always of animal food principally, and a small quantity of vegetables; to which was often added a little spirits or porter; and, notwithstanding the vast difference between the stimulating powers of these two meals, the morning pulse is inferior to the one after dinner by only two beats; the difference would have been ten or fifteen, had the case been reversed, at least I am induced to think so from some experiments which I afterwards instituted directly to ascertain the point. In the evening the pulse was reckoned after supper, light, indeed, but certainly equal to the breakfast in stimulant power, and in some measure more so, because spirituous liquors very generally supplied the place of coffee. Notwithstanding this, the evening pulse is found to be, on an average, nearly eight beats lower than the morning one; a difference by no means inconsiderable, and sufficiently warranting the conclusions I have drawn from it.

The following table was drawn up, in order to ascertain the effects which moderate exercise in the morning would produce on the pulse, reckoned, however, in such a manner as not to be immediately affected by it.

TABLE.

A walk of three miles before breakfast. Pulse after breakfast.	Pulse after dinner, as usual.	Pulse after supper, as usual.
74	76	62
81	78	62
80	75	62
83	78	64
80	75	66
78	74	64
Average 79.33.	Average 76.	Average 63.3, &c.

3. My experiments have not yet enabled me to ascertain, with precision, the hour when the pulse begins not only to be actually more rapid, but also to acquire a greater capability for action, manifested by the exhibition of any stimulant. This, with myself, perhaps takes place about 3 P. M. (A. M. ?); but there is every reason to believe that the time varies with the individual, the season, climate, and perhaps, though this for many reasons is improbable, with the mode of life.

4. This increased capability for action, occurring regularly in the morning, is even of greater importance than the *actual state*

of the circulation; the former may be made the subject of very delicate experiments; the latter we know to be exposed to a thousand variations, from causes which have been in part developed. While experimenting on this subject, there occurred to me a case, altogether of so interesting a nature, that I cannot refrain from giving it in detail. It affords a remarkable proof of the truth of the doctrines brought forward in this essay; at the same time, independent of this consideration, from the rarity of its occurrence, it appears to be worthy of record.

M. C. aged 4.—The account we received from the parents of this child was, that she continued in good health until about six months after birth, when a blueness of the surface was remarked, particularly on any exertion, and accompanied with perpetual difficulty of breathing. In this state she continued to grow, and became remarkably tall of her age. She was observed, however, to get daily worse; that is, the paroxysms of threatened suffocation became more frequent, during which the whole surface of her body appeared almost black. A strong beating was constantly to be felt in the region of the heart, and she was sometimes convulsed; muscular debility great. At her death she measured three feet four inches. The body continued warm for five hours after death.

Dissection.

1. *External appearances.* The countenance, feet, and body, were not all discoloured; the arms and fingers retained their usual dark colour.

2. *Thorax.* The lungs seemed sound, though dark-coloured; heart perhaps larger than usual; *foramen ovale* pervious, and pretty large. The *aorta* arose from both ventricles in such a manner, that the ventricles communicated with each other, and with the cavity of the vessel, which was much enlarged; its *vasa vasorum* distended with red blood. From the right ventricle there arose a pulmonary artery, very small in its diameter, (perhaps the size of a small goose quill) and feeble in its coats; it possessed all the characters of the artery, such as its valves and division into two branches. Not the smallest vestige of a *ductus arteriosus*; it seemed never to have existed. The passage from the left ventricle into the artery, common to both, was more direct than that from the right. Every where the arteries were full of dark blood, not coagulated.

3. The abdominal viscera were of a very dark colour. The head was not examined.

Before leaving the house, the father of the child related to us a circumstance to which we all paid particular attention. The child, he informed us, had always been very fretful and peevish, (perhaps from indulgence,) and apt to have frightful paroxysms when spoken to harshly; but these happened much more frequently in the morning; indeed he observed that she was always ill in the early part of the day; so much so, that the smallest exertion, an angry word, or even a cup of tea, would instantly bring on a paroxysm, each time threatening destruction. This tendency to these fits decreased from morning, till about 5, P. M. from which time she generally continued well till bed-time. The father detailed this with great minuteness, and seemed to wish for an explanation.

It will be easy to anticipate to what cause I would ascribe this increase of disorder in the circulation, during the forenoon. So disproportionate was the pulmonary artery to the aorta, it was with difficulty that life could be carried on; but, in the morning, when the susceptibility of the arterial system was greater, the inequality must also have been increased to such a degree as almost to have destroyed life. I may also remark that she died about 9, A. M.

5. To be convinced that an opinion, very different from that now brought forward, is the one more generally received, we have only to turn to the writings of a few celebrated physiologists. Thus, for example, in the "Physiologie Positive" of Fodéré, a work of merit, we find the following remarkable passage: "Relativement à la difference du jour et de la nuit, on remarque que le pouls de l'homme adulte bat de 60 à 65 fois par minute, au commencement du jour, et qu'il va continuellement en augmentant jusqu'à battre 80 fois dans le même temps, chez les plus excitables, sur la fin de la journée; dans la nuit, les pulsations diminuent de nouveau, jusqu'au matin, où elles se trouvent revenues insensiblement au nombre de 60 à 65." * In order to have an approximation to the truth, we have only to reverse the above statement. If we regard as accidental circumstances the accelerations of the pulse, occasioned at various times by our diet, and that, too, according to the caprice of the individual, the pulse shall be found gradually to diminish in velocity from an early hour until midnight, and generally later. This difference I have found to be great in winter, but less, though still existing, in summer, as the above experiments, per-

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* Essai de Physiologie Positive, Tom. I. p. 190.

formed during that period, sufficiently prove; and from this I am inclined to believe, that heat has no inconsiderable share in the production of the phenomenon, though totally inadequate solely to produce it. This gradual excitation in the number of the pulsations Fodéré conceives to be owing to the combined effects of exercise, of the action of the internal and external senses, of light, heat, purer air, &c. Violent exercise towards evening will certainly produce a considerable excitation of the pulse, but less than what takes place in the morning, in consequence of the same degree of exercise; neither will any degree of heat, nor quantity of food, raise the pulse in the evening to the height at which it is in the morning, almost without any such excitants, provided they be not used in an immoderate degree.* Thus, after fasting till 8 P. M. I found my pulse to be 58; at 9 P. M. after eating a moderate dinner, pulse 58.

The common mode of experimenting with unequal quantities of food, taken at various hours of the day and night, was evidently insufficient to establish the principle in its full extent. I endeavoured, accordingly, to submit to a particular regimen, and examine the state of the pulse under the influence of a diet always similar at every hour of the day. This decisive experiment could not be pushed any length; so difficult a matter did I find it to break through, at once, those habits which a great number of years had firmly established.

6. "All these things (continues Fodéré) singularly facilitate the return of the venous blood towards the heart; and this is one of the causes of the evening paroxysm which takes place in all fevers." But, having denied that any such gradual increase of the pulse exists, we need scarcely stop to say, that this cannot be one of the causes of the evening paroxysms in fever.

This opinion appears to be borrowed from the celebrated Cullen. That author, however, increased the facility of explanation, by describing two exacerbations or augmentations of the pulse; one about mid-day, the other towards evening. This explains, in his opinion, the occurrence of the double paroxysm in hectic fever.† The whole is a pure *hypothesis*, which seems partly to have arisen from supposing that the pulse was governed by the same laws in health and disease. So far as my observations go, the order of nature in disease is entirely reversed, and the observations of Cullen, Fodéré, and others, are applicable only to the unhealthy. That this acceleration of the pulse (which is really an aberration from the regular laws of nature) actually happens in fever, the testimony of Cullen, generally so accurate in the observance of disease, will sufficiently establish; and that this universally takes place in phthisis, we have, unfortunately, too many opportunities to verify.* It was from the sinking of the pulse towards evening that I ventured, independent of other circumstances, to prognosticate favourably in the case of a child labouring under typhus fever. It was this which induced me to hope that some of the functions had begun to resume their natural order, and that recovery was a probable event. Nor was I deceived; by proper attention the patient from that day rapidly amended.

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* In experimenting on this point, care must be taken that the acceleration of the pulse, occasioned by indigestion, be not confounded with the natural healthy pulse, produced by the simple stimulation of the food.

† First Lines of the Practice of Physic, Vol. I. c. 862.

7. The physiologists who invented the various diurnal accelerations of the pulse, one, two, or more, just as suited their fancy or necessity, will perhaps find little difficulty in explaining the gradual diminution of the pulse, should the phenomenon appear to them to be correctly stated. With me, I confess, the cause is yet excessively obscure, because I have not found it connected with any circumstance, to the influence of which I might ascribe it. Experiment showed me that no previous exhaustion from labour, excess in food, drink, &c. rendered the diurnal diminution of the pulsations more evident than usual; the want of food, perhaps, hastens and increases it, but certainly does not prevent or retard it. Neither is sleep the cause of the restoration of the susceptibilities and velocity of the morning pulse, since the negation of that does not at all destroy the *excitability*† of the sanguiferous system, as manifested by the application of the accustomed stimulants.

August 30th 1813, the day being moderately warm, I walked, between 1 and 11 P. M. a distance of nearly 40 miles. Not having much appetite, retired to rest about 1 A. M. after drinking a little coffee, but slept none, perhaps owing to over-fatigue. Next morning (31st) about 7, my pulse was 80 and rather feeble; after breakfast, before which I took a small glassful of spirits, my pulse rose to 104. I was not feverish, and performed a journey that day of 27 miles, at a tolerable pace.

To what, then, are we to attribute this daily diminution of velocity in the functions of the sanguiferous system? Its existence as a law of the sanguiferous system, has been demonstrated experimentally; and I shall endeavour to shew, that a similar revolution daily takes place in several other functions of the human frame. To this conclusion Dr Cullen arrived, merely from observing the daily returns of sleep and watching, of appetites and excretions, and the changes which regularly occur in the state of the pulse. Of these changes he had, however, no clear ideas. When he proceeds to combine this diurnal revolution with the phenomena of fever, he unquestionably offers a conjecture, extremely probable, ingenious, and perhaps original; but unfortunately there results from this fine idea, nothing but conjecture and hypothesis, attributable principally to an incorrectness in the observance of the phenomena.

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* Beddoes' Essay on Consumption, p. 252.

† I use this word merely to avoid circumlocution.

8. It required few experiments to convince me, that animal food raised the pulse much more than vegetable; the excitation of the pulse by wine is still greater, and that from spirituous liquors greatest of all. By these circumstances, but more especially by diet, the regularity in the diurnal revolution in the pulse, is, as might have been anticipated, much disturbed. But it was impossible to perform these experiments without remarking, that something similar happened to various other functions. Thus, beyond all doubt, our perceptions in the early part of the day are clearer, our minds more acute, our whole intelligence more active. The functions of the stomach seem also much stronger at this time than towards evening. Feverish, restless nights, are the invariable attendants on late meals, which injure in the *ratio* of their quantity.

I have repeatedly remarked, that digestion went on more easily in the morning than in the evening. Three or four times have I been induced (sometimes compelled), whilst following some favourite sport in the country, to defer the taking food until evening; a greater or less degree of fever and restlessness, in proportion to the quantity of food taken, has uniformly followed such indulgence.

It was long with me a problem difficult of solution, why digestion should go on laboriously during the evening, when the actions of the muscular system were almost entirely suspended; more especially when I recollected, that the opinions of most, perhaps all physiologists, led us to conclude, that rest greatly favoured the digestion of our food. The objection, that during sleep, in which state the repose of the body is complete, digestion, nevertheless, is generally ill performed, seemed at first insurmountable; and it is not a little remarkable, that those physiologists who have so repeatedly stated the fact, have as constantly failed to note the objection. On observing that practical men expressly stated the necessity of rest for the right performance of the function of digestion, and experiencing daily the truth of the observation, I was convinced that the above objection was rather inexplicable than hostile to the opinions of physiologists, which I then, and still do consider as strictly correct. But this phenomenon is no longer difficult of explanation, if a daily revolution in the functions of the stomach be demonstrated by experiments, or even rendered probable by analogy.

On this subject, the opinion of those, whose profession it is to train men to the performance of great muscular feats, when they speak the truth, is of much more consequence than that of any medical man. Experience has taught them, that the evening is not a proper time for the digestion of the food; and accordingly we never find any substantial meal taken by their pupils after 5 P. M.; indeed they lay it down as a rule, that on going to bed, the stomach should have as little to do as possible. Thus it is recommended* to sup about 9 o'clock on a chicken, or some food that is nourishing, not gross. In another place †, you must retire early to rest, on a supper of runnet-milk, or milk-pottage. Again ‡, two meals a-day, viz. at 8 A. M. and 5 P. M. But these hours are rather later than the ones laid down by Jackson, who says, § "they breakfast upon meat about 8 o'clock, and dine at 2. Suppers are not recommended, but they may take a biscuit and a little cold water about 8 o'clock (I never heard of a more moderate supper), two hours before they go to bed."

The object kept in view during the training, is to enable the human frame to acquire the utmost degree of vigour consistent with health. To accomplish this, they employ the organs at the time when they ought to be employed, that is, during the early part of the day. "The exercise is always begun early in the morning, in summer at five; in winter at half-past six, or as soon as it is light. We prefer rising early in the morning, indeed it is indispensable."

Strictly speaking, this increase in the functions of the body, may be more properly called an augmentation and diurnal revolution in the functions of the nervous system. But this is too indefinite a term; no two individuals attaching precisely the same meaning to it; and we shall therefore consider the above facts, brought forward by Mr Jackson, as proofs of an increase in the powers of the muscular system, without offering any conjecture on what that peculiarly depends; whether it be connected with increased energy of the brain, or be totally independent of it.

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* Sinclair's Code of Health, Vol. II. p. 163.

† Ibid p. 112. ‡ Ibid p. 104. § Ibid p. 94.

I shall close this part with a single additional observation. It is this: The regular decrease in the powers of the stomach is not dependent on any previous exertion of that organ, for I have found that a dinner taken at a late hour, with or without previous exhaustion, was always digested painfully and laboriously, with feverish nights, distressing dreams, and, instead of refreshment, further exhaustion. This arises not from the food stimulating at that time the system more powerfully than in the morning. If we may judge from the pulse, it stimulates the body much less at midnight than at nine in the morning; but it arises from this, that the powers of the stomach are more languid; it does not digest the food taken into it; and should a feverish night follow, it is neither wonderful nor inexplicable. Hence we see the propriety of no function being much employed during the evening; not because it will greatly excite the pulse, and so produce fever (ridiculous idea! the excitation of the pulse does not produce or constitute fever); on the contrary, the stimulation is actually less, it is almost as nothing, so far as regards the pulse; but the phenomenon is occasioned by this, that all the organs are less powerful, less capable of exertion; in short less able to perform their functions, or undergo fatigue. A remark made by Cullen, also illustrates in some degree the doctrine I have brought forward. "It is indeed to be observed, that in almost every person the taking of food occasions some degree of fever; but I am persuaded this would not appear so considerable in a hectic, were it not that an exacerbation of fever is present from another cause; *and accordingly the taking of food in the morning has hardly any sensible effect.*" Here we see what so seldom happens, all the facts throwing light on the doctrine, and it on them; for although the morning be the time when the action of food over the pulse is greatest, yet is there no febrile state excited. On the contrary, in the evening, as Cullen has remarked, a febrile paroxysm occurs in hectic, independent of food, aggravated perhaps by its presence, but whose real cause has totally escaped him.

9. Nor does this law seem confined to the functions of the brain, stomach, muscular, and arterial systems,—it extends, if I mistake not, to that of the lungs. Dr Prout found, "that the quantity of carbonic acid gas, formed during respiration, is not uniformly the same during the 24 hours, but is always greater at one and the same part of the day than at any other, that is to say, its *maximum* occurs between 10, A. M. and 2, P. M., or generally between 11, A. M. and 1, P. M.; and its *mini-*

mum commences about half-past 8, P. M., and continues nearly uniform till about half-past 3, A. M."[*] The same gentleman observes, that the quantity of carbonic acid given off during respiration, bears no proportion to the numerical state of the pulse; in fact he imagines that most carbonic acid is given out when the pulse is least frequent. My own experiments undoubtedly disprove this idea. The greatest quantity of acid, according to Dr Prout, was given off during the forenoon, when the pulse is, in general, higher, and always more easily excited by any exertion; we may almost say, that the capabilities of the arterial system are at that time greater; and the importance of this remark, as it regards *secretions*, must be obvious. Besides, from what I can judge of the tables given by Dr Prout, they refer principally to the afternoon and evening, unless some of the tables be wrong marked. Notwithstanding this, I am much inclined to agree with him in this, that the quantity of carbonic acid given off during respiration is not particularly connected with, at all events not dependent on, the state of the circulation. His experiments on the state of the respiratory organs after exercise—after the taking of spirituous liquors—during a mercurial course;[†]—in short, after every thing which could excite the circulation, show this in a decided manner. These experiments, however, require repetition. Many of them are too scanty to enable us to draw any certain conclusion from them, whilst others are contrary to all analogy.

The opinions contained in the excellent essay of Dr Prout do not seem to invalidate the conclusions which I have ventured to draw from my experiments: they amount to this—that all the functions, or at least many of them, are more vigorous in the morning than in the evening; that their capability for action is certainly greater; and that this increase in the functions commences at a much earlier hour than is generally imagined.

Were it lawful for me to speculate, in this experimental age, I would venture to support an opinion, at present, I allow, somewhat antiquated, and very *unfashionable*, that early rising may be conducive to long life, as it most certainly is to the perfect enjoyment of all our faculties. It was from repeated violations of all these dietetic maxims that I first perceived their importance; perhaps by a similar experience alone can others be convinced of their value.

Before concluding this section, it is my duty to observe, that, on mentioning the results of some of these experiments to a medical friend, he assured me that experiments, performed by him about ten years ago, led to conclusions which were, in his opinion, extremely similar to mine. This circumstance, he observed, was very satisfactory to him, as my experiments had been performed, and my conclusions drawn, without any communication of ideas: it will be equally so to me, if I find the results exactly to correspond;—results of experiments performed by individuals so opposite in habits, temperament, and opinions.

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[*] Annals of Philosophy, Vol. II. p. 330.

[†] Dissertatio de Copia Acidi Carbonici, &c. Andrea Fyfe auctore, Edin. 1814.

(*To be continued.*)

V.

Observations on the Impropriety of returning the Hernial Sac unopened into the Abdomen. By HENRY EDMONDSTON, Surgeon, Newcastle upon Tyne.

MOST of the following observations were written between three and four years ago, but I could not prevail upon myself to request that they might be inserted, as the subject had frequently undergone minute discussion; and I was apprehensive that they did not possess interest sufficient to repay the trouble of perusal. Probably this defect will still be found to attach to them. But as I find that the question has been revived in a late number of the Journal, and as the inquirer differs in one material point from the illustrious individual whose high authority he follows, I am induced respectfully to solicit a place for them in your valuable publication.

As it is confessedly an object of great importance in the practice of surgery, to determine whether, in the operation for strangulated hernia, the sac should be opened, or the bowels returned unexamined into the abdomen, I presume a few observations on the subject will not be deemed wholly destitute of interest. The method now most generally practised, is to lay open the sac, previous to the reduction of the intestine. Yet there are some authors, and these of no ordinary authority, who recommend strenuously the opposite plan of relieving the stricture merely, and returning the sac unopened into the abdomen.

One of the ablest practitioners which perhaps this or any other country has ever produced, is decidedly of this opinion. As his name stands deservedly at the head of the profession, and consequently as his precepts may influence many of those who are educated at the medical school of Edinburgh, it may be of some moment to investigate the grounds of this practice.

For this purpose I shall endeavour to analyze the account of the practice by Dr Monro, senior, as it appears in the appendix to the observations on crural hernia, by his son, Alexander Monro, M. D.

I may observe, that, on entering upon such an analysis, I feel myself actuated by no other motive than that of wishing to ascertain the proper rule by which the performance of the operation should, in general, be regulated. To oppugn the opinions of Dr Monro, merely for the sake of cavilling, would not only be jejune and unavailing, but it would be repugnant to those sentiments of profound respect and veneration, which I entertain for his professional name and character.

The Doctor commences his reasoning by assuming for his premises, that "if wounds be made into the abdomen of sound animals, and that a portion of the alimentary canal, as large as that which is often contained in a herniary sac, be handled and exposed to the air, the animals frequently die;" and in order to shew that "the wound of the tendon or peritonæum contributes little or nothing to the fatal event," * he states a variety of reasons deduced from the result of Haller's experiments, and of his own; and among others, he says, "In like manner, I have seen the vaginal coat of the testicle laid open by incision; but the dressings (mark, the *dressings*) having slipt from between the lips of the incision, these grew together without producing such a degree of inflammation as seemed necessary to effectuate a cure, so that there was a necessity of tearing them asunder, to introduce dressings, and to admit the air."

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* It may here be noticed, that this is the principal point on which the Inquirer, in the 39th number of this Journal, differs from Dr Monro. The writer says, "every surgeon is sensible that, in all instances where death takes place after the operation, it is owing to inflammation of the intestines, or of the peritonæum, (the latter most frequently) having supervened to its performance. This applies to those cases where the operation is performed under the best auspices, and where it has been resorted to before gangrene takes place in the strangulated portion of intestine."

This, if not a gratuitous, is at least a bold assumption in argument. Whether it is consonant to fact I am really incompetent to decide. The writer seems here to exclude from his calculation altogether, the effects which the state of things previous to the operation may have produced, and which the operation, no matter how performed, is perhaps incapable of averting.

If this instance proves any thing, it certainly does not prove that the air itself, or even the handling, was capable of producing the requisite degree of inflammation, but that it was necessary to supply mechanical stimuli (dressings) to effect that purpose. From these, however, and similar considerations, the Doctor draws the certain conclusion, " that the danger of the operation of hernia is not owing to the mere division of the tendon or of the peritonæum, but that it depends chiefly, and almost solely, on the handling of the bowels, and the exposure of these and of the inner side of the sac, to the air."

Because persons die from wounds of the abdomen, or because any one particular cause is insufficient to account for the fatal event, it does not follow necessarily and legitimately that another circumstance must produce it. If indeed there was no other circumstance present, we should be under the necessity of referring the fatal event to such a cause, for lack of a better; but where wounds of the abdomen have been inflicted, and the violence consequent on such wounds has been committed to the contained and containing parts, we have no need to search for causes whose adequacy appears to be more than problematical.

That rough handling and long exposure to the air may prove dangerous, cannot be denied; but that even a considerable degree of these, and still less that the violence and exposure which the parts usually suffer during the operation for hernia, where the sac is opened, are the chief and sole cause, few will perhaps be inclined to subscribe to. The delicate handling of the bowels, for example, during the operation, cannot certainly be put in comparison with the injury which they sustain from the hard unyielding pressure by the tendinous fibres which form the stricture, or from the pressure of hardened feces.

The argument upon which Dr Monro establishes his opinion proves at best very little, for one might assert with safety, in opposition to what he adduces, that " wounds have been made, and with violence too, into the abdomen of sound animals, and a portion of the alimentary canal, even *larger* than that which is often contained in a herniary sac, has been handled and exposed to the air, and the animals have frequently *recovered.* It is unnecessary to cite instances in support of this position. —they are of daily and familiar occurrence. One of the most recent and satisfactory, is that given by Mr Hague of Ripon in the 18th number of the Edinburgh Medical and Surgical Journal. He says, " On my arrival, which was *little more than an hour* after the accident had happened, I found the poor lad in a very distressing situation; the *great arch of the stomach* and the *whole of the intestinal canal*, duodenum excepted, contained within the abdomen, having protruded through the wound." If Dr Monro's principles were invariably, or even generally correct, how happened it that this patient survived? The exposure here continued at the very least five times as long as that period, generally occupied by the particular stage of the operation for hernia, where the bowels are brought into view, and the surface exposed, and consequently the handling were beyond all calculation greater. From these contradictory facts, therefore, which are equally true, no general principle can be deduced; the only inference that would seem to result, is, that the handling and exposure are not the sole, nor even the chief causes of the danger. Yet Dr Monro, in corroboration of his opinion, says, "nay, in some cases where the operation of the hernia had proved fatal, I observed, on opening the abdomen, that portions of the intestine, and likewise of the peritonæum, at a distance from the herniary sac, were much inflamed, although the patient had not, before the operation, complained of pain, except at the place of the rupture." This cannot surely be intended to illustrate the injurious effects of handling and exposure, because, admitting that, in these cases, the sac had been laid open (which, by the bye, does not appear) the parts of the intestine, at a *distance* from the herniary sac, could not have been touched or exposed, for, in no case of opening the sac, by even moderately skilful hands, can more of the intestine be handled or exposed, than what actually constitutes the protruded portion, or the parts in its very immediate vicinity.

This, then, so far from proving any thing in favour of Dr Monro's principles, would seem to demonstrate that these deleterious consequences arose, in a great measure, independently of the exposure during the operation of the parts composing the hernia, or at least they are not to be explained altogether by such a circumstance. The Doctor then proceeds to shew how inconsistent with each other are the opinions and practice of surgeons who advise the opening of the sac. He says, "for if the mere stricture made by the tendon is, in the generality of incarcerated ruptures, not only a sufficient, but the primary, and indeed the sole cause of the symptoms, and of all the mischief;" (Pott) "why, after the skin is divided, do surgeons first lay open the herniary sac its whole length, and then cut the tendon, instead of merely cutting the stricture made by the tendon, without opening the sac, and then reducing the bowels by pressure, posture, and agitation, if the latter should ever be necessary?"

That Mr Pott considers the stricture made by the tendon as the sole cause of the symptoms, would scarcely seem to preclude the propriety of opening the sac, or involve him in inconsistency by recommending that measure, because it must be granted, that the stricture made by the tendon, allowing it to be the sole cause of the symptoms in hernia, may produce such a condition of the parts constituting the hernia, as the mere division of the tendon and relaxation of the stricture shall be insufficient to remove. Can the division of the tendon, for example, avert the progress of gangrene? Can it disunite those adhesions of which dissection affords us so many proofs?

With respect to the objection of surgeons laying open the herniary sac its whole length, before cutting the tendon, it seems to be, under any circumstances, one of little weight, but it is rendered of no avail, when it is considered, that the great and primary objects of this step of the operation are, first, to ascertain the state of the contents of the sac, and, secondly, to disengage any adhesions which the inflammation may have occasioned.

These objects are animadverted on by Dr Monro, for the purpose of shewing that they prove more than is necessary, for says he, "they ought to prevent surgeons from attempting, in any case, at least from attempting, in most cases, the reduction of a hernia." "Yet," he continues, "nothing is more common than to see surgeons doing every thing in their power to reduce a hernia, and, in a few minutes or hours thereafter, instead of taking off the stricture by cutting the tendon, laying open the herniary sac, as if the reduction of the bowels would otherwise have been unsafe. Surely no reason can be given why the reduction of the bowels should be safe before the tendon is cut, but unsafe after it is cut." It is not to be conceived how it could have escaped a person of this distinguished professor's sagacity and penetration, that the reduction of the bowels before or after the cutting of the tendon, is, abstractedly speaking, of no moment whatever. A surgeon undoubtedly tries every means in his power to reduce the hernia, without exposing his patient to the pain and hazard of an operation, and it is not until the measures employed for this purpose have failed, or the hernia has continued strangulated for some time, or that unfavourable symptoms either have ensued, or are likely to ensue, that he is justified in resorting to the knife. But, assuredly, his having recourse to this severe expedient is no reason for not attempting the reduction by every possible means that can be suggested, or for not opening the sac. On the contrary, it is the strongest reason for doing the latter; because the failure of the other means is at once a satisfactory proof, that a serious obstacle opposes the return of the gut, and the continuance of such obstacle or stricture, is a sufficient ground for apprehending mischief to the parts, and, consequently, for ascertaining whether such mischief has actually taken place, and may be prevented or counteracted.

If the replacement could be effected by the taxis alone, the strongest presumption would necessarily be afforded, that the stricture had not been very severe, and consequently, that the protruded portion had not sustained any material injury, and hence the dangers from returning it unexamined would be almost entirely obviated. But, if the stricture has resisted every means, a very different inference must be drawn, and a different plan of treatment must be pursued. The mere point of time, therefore, is of no importance in reasoning upon this part of the subject, for, although in operating we may open the sac of the very hernia, which, a few hours, or even minutes before, we would have thought ourselves fortunate in being able to reduce by the taxis, this constitutes no valid ground of objection to the opening of the sac, because the inadequacy of the palliative means to accomplish the reversion is the principal cause of suspicion, and, consequently, our chief reason for inspecting the state of the parts.

Besides, every one is aware, that, in some cases of hernia, either from peculiarity of constitution, or the severe nature of the case, the inflammation is apt to run an unfavourable course very rapidly, and what might be safe at any given period, might not be equally so even a few minutes, still less a few hours, afterwards.

The Doctor next proceeds, " If it be certain that the bowels are mortified, the necessity of opening the sac is evident; that we may give our patient a chance of life, though to be attended with most uncomfortable circumstances. But, if there be the smallest chance that the inflammation may terminate without mortification, it is equally certain that nothing can be so pernicious as opening the sac, and that the bowels ought to be returned without exposing them to the air." Now these are the very points about which the chief doubt exists. If it could be ascertained with precision, that there was a chance that the inflammation may terminate by resolution, then the necessity for opening the sac would perhaps be partly got rid of. But it may reasonably be asked, How is this to be done if the sac be returned unopened? What certain test have we of the soundness of the protruded bowels?

The calculation upon which the Doctor enters for the purpose of illustrating this branch of the subject, proceeds upon data which the general result of cases will scarcely be found to authorize, namely, that in all the operations for hernia, even under the most favourable circumstances, not more than one patient in four recovers.

In order to set the matter at rest, by bringing it as he conceives to the test of experiment, the Doctor has detailed four cases in which the operation was, by his direction, performed successfully, without opening the sac. But this is not the point at issue. An operator, with the tenth part of Dr Monro's experience, could easily bring forward double the number of cases where equal success had attended the operation, when the sac had been opened.

The matter, therefore, remains precisely where it was. But the great purpose to be attained is, that, while we remain ignorant of the incontestible proofs of the condition of the protruded parts, it is necessary to establish a practical rule of procedure; in other words, to determine whether the sac should or should not be opened in the generality of cases. If the mode of operating insisted on by Dr Monro is to be the rule by which surgeons are to be guided, it is incumbent on him to specify the circumstances by which it may be known when it is safe, and when it is unsafe, to return the sac unopened into the abdomen. This I apprehend it is impossible to do.

Having thus attempted to prove that the practice which Dr Monro recommends can hardly be justified, even upon the principles and reasoning which he employs, it remains for me to make a few observations on those circumstances which appear to render the opposite plan of treatment more eligible.

The destructive consequences arising from the introduction into the cavity of the abdomen of any part of the viscera in a gangrenous state, are too obvious to require being pointed out. Now, we possess, as was already mentioned, no means short of opening the sac, by which we can ascertain, with any degree of precision, the state of the contained viscera.

We may, it is true, infer, with some probability, from the appearance of the sac, from the state of the pulse, and other circumstances, that gangrene has or has not supervened; but still there is room for doubt. The possibility, therefore, of some portion of the viscera being returned in a mortified state, must be admitted; and if it can occur in one instance, it may occur in many. This consideration, therefore, is in itself a sufficiently satisfactory reason for opening the sac.

Although the prolongation of life be attended by the most unpleasant and distressing consequences after a gangrene of a portion of the bowels, and a preternatural opening have taken place, yet we are not warranted in exposing the patient to certain death, by returning the bowels in a mortified state, unless it can be clearly shewn that the danger from handling and exposure is fully as great as the other:—greater it cannot be.

It might here be asked, what would the state of that operator's feelings be, who should discover, on dissection, that, had he opened the sac, he might have protracted the life of his patient? Another, and not an unimportant reason for examining the state of the parts contained in a herniary sac, is, that such examination furnishes us with some knowledge, by which the subsequent treatment may be regulated; for it is evident that the difference in the after-treatment of a part which is in a state of incipient inflammation, and that bordering upon gangrene, independently of all the intermediate stages of these extremes, would be very considerable indeed.

The twistings and adhesions, which sometimes form between the bowels and the neck of the sac, and among the protruded bowels themselves, constitute another powerful reason for laying open the sac. Dr Monro, after reprobating any attempt to separate adhesions, says, "but if, instead of this, the reader shall, upon the authority of Mr Pott, judge it advisable in all cases to attempt the separation of adhering parts, it will surely be time enough to open the sac after we have taken off the stricture, by cutting the tendon, and found that the bowels cannot be reduced by pressure, because they adhere to the inner side of the sac."

Here it may be remarked, that the perfect reduction of the bowels, or their extrication from the ring, after the tendon has been divided, is no assurance of the release of the contents of the sac; these may adhere to each other and to the sac, and the patient may die unnecessarily, after having undergone the operation. Mr Pott says, "I have seen the intestines very firmly adherent to each other, to the sac, to the omentum, and to the testicle; but never in such a state of adhesion as to be incapable of being returned." But, let us make the supposition that a hernia may occur composed of omentum and intestine; that the former may have been protruded in such quantity, and under such pressure, as to occasion the loss of its vitality; but that the latter shall be down to such an extent only as shall cause a certain derangement of its functions, but without exposing it to any thing more than a high degree of inflammation,—what would be the consequences of each of the operations we have just been discussing? According to the method recommended by Dr Monro, the contents of such a hernia would be returned into the abdomen uninspected, as there would exist here no such extreme derangement of the intestinal canal, as, upon his principle, would justify the opening of the sac. The consequences of returning the mortified omentum into the abdomen, it is unnecessary to describe; and thus the patient might possibly lose his life without any adequate cause. On the contrary, by laying open the sac, the state of the parts is at once ascertained;—the diseased portion of omentum is detached;—the sound intestine is returned, and the danger is rendered comparatively trivial; or at least it constitutes the *minimum* of that which every person must incur who has the misfortune to undergo the operation.

It is almost impossible to determine, from the appearance of the sac in any given case, the exact state of the contained bowels, the rapid progress which the inflammation frequently pursues rendering this object wholly unattainable. In addition to all these arguments, we have the well-established and incontrovertible fact, that the operation for hernia, as generally performed by opening the sac, is found not to be more dangerous or fatal than where the bowels are restored unexamined. Nor have any decisive proofs of the bad effects of handling and exposure to the air ever been brought forward.

Finally, the general effects produced on the system by the strangulation,—the inflammation,—the interrupted, and frequently the inverted peristaltic motion of the bowels,—the fever, and general derangement excited,—peculiarity of constitution,—improper treatment,—and perhaps various other inexplicable circumstances,—are fully sufficient to account for the fatal event, without having recourse to unproven or hypothetical reasoning.

The point in question has been very ably investigated, and, in great measure determined, by Dr Monro, the grandfather, in the Edinburgh Medical Essays, Vol. V. art. 21. This valuable paper on the subject I fell in with but very lately. With some exceptions, it embraces almost every thing that has been since offered on the question, and indeed almost supersedes the use of any arguments. I cannot, therefore, conclude this inquiry more satisfactorily than by quoting his opinion. He says, "When the sac is laid bare, it ought to be considered whether it is to be left entire, and pushed up into the ring after the bowels are reduced, or if it should be then laid open? *Circumstances must determine this.* If the disease is recent, with the sac thin, and not folded into wrinkles, or straitened where it is coming through the passage in the muscles, or grown to any other part;—if the bowels are sound, and in no danger of gangrene, or are not grown to the sac;—if the liquor in the sac is limpid, and no fetor or erosion is to be observed; if all these circumstances are present, the reduction of the sac entire will be of service to block up the passage, and to prevent the viscera from being exposed to the action of the external air. Where these circumstances do not meet, the sac *ought to be opened* for *very obvious* reasons. The wrinkled or contracted sac may continue the strangulation after the ring of the muscle has been cut;—the sac or bowels, fixed by concretion, will not reduce;—an opened gut will let out the *ingesta*, and a mortified *omentum* will slough off into the abdomen, from which there is no exit; and, stagnating there, they will corrupt more, and do great mischief. The same effect may be expected from the liquor in the sac."

Newcastle upon Tyne, Sept. 1814.

VI.

Case of Hydrophobia, which occurred in the Regimental Hospital of his Majesty's 1st Foot, or Royal Scots. By Mr George Ballingall, Assistant-Surgeon.

Trichinopoly, 23d February 1813, 7, A. M.—Sergeant James Clarke, aged 39, a tall, stout, well-made, healthy man, and evidently possessed of a good share of resolution, in attempting to swallow his usual dram, previous to going out yesterday morning, felt a peculiar indescribable reluctance to the liquid, and could not prevail upon himself to take more than one-half of it. Again, in attempting to wash his face, preparatory to evening parade, the approach of the water threw him into a violent state of agitation, and he was obliged to have it removed. Although now distressed with the most urgent thirst, he cannot be prevailed upon to attempt swallowing any fluid, the approach, and even the mention of it, producing violent spasms of the muscles of the neck and throat; which spasms are preceded by a peculiar uneasy sensation about the scrobiculus cordis, and a kind of sobbing or inclination to sigh, attended also with severe pain in the head. His eye-balls appear turgid, and a degree of furor is depicted in his countenance; pulse about 110 in the minute, and rather small; heat natural; tongue white and moist; belly regular.

Mittr. Sanguis e brachio ad. ℥xl.

9. A. M.—The blood was drawn from a large orifice, and the patient kept in an erect posture; he complained of exces-

sive languor and faintness during the operation, but no deliquium supervened; pulse, (immediately after the bleeding) about 88, small, and occasionally intermitting. The near approach or agitation of any fluid in his presence still produces a recurrence of the spasms, although he can bear to look at water when kept at a distance. Shews no reluctance to look at himself in a mirror, nor any aversion to light; neither does he object to solid food, but has no inclination to eat. Pulse upwards of 100, with intermissions; turgidity of his eye-balls diminished; pupils dilated, but sensible to the accession of light; skin moist. At this hour, the patient was visited by Dr Anslie, Mr Peyton of the 14th N. I., and Mr Campbell of the 8th N. C. when it was agreed to persist in the bleeding, should circumstances render it admissible; and in order that its effects might be distinctly marked, the exhibition of any medicine was purposely abstained from.

11. A. M.—Has forced himself, evidently with great exertions, to swallow about half-a-pint of milk and water, through a tube affixed to an elastic gum-bottle; this he describes as producing the most grateful sensations to his stomach, and he has every inclination to take more, but is afraid to renew his efforts. The turgidity of his eye-balls is now much diminished, and his countenance altogether more placid; pulse not more than 84, small, with intermissions; skin cold and clammy.

2. P. M.—Has had several attacks of the spasms within the last two hours, and one particularly severe occurred on seeing a bason of sago which was offered to him. The pulse, except when occasionally raised by the recurrence of the fits of spasms, has uniformly continued to sink since nine o'clock, and is not now more than 74, with occasional intermissions; pupils of his eyes largely dilated, and the eye-balls seem a good deal fixed; skin covered with a clammy sweat; has had one evacuation by stool since last report.

4. P. M.—Spasms have latterly become more frequent; he renewed his efforts to swallow, and succeeded in getting down a little milk and water; the remainder was rejected with the utmost horror. At the suggestion of Dr Anslie and Mr Peyton, who again visited the patient at this hour, and encouraged by the success of Mr Tymon's practice, and the case lately made public on high authority, (Dr Shoolbred of Bengal,) it was agreed to give the bleeding a further trial, and a vein was now opened in his left arm, from which about 16 or 18 ounces of blood were taken away. He expressed the utmost reluctance to be bled, and struggled so much during the operation, that the quantity could not be exactly ascertained, nor its effects dis-

tinctly observed. The pulse at one time fell so low, as to be scarcely perceptible at the wrist; and, towards the close of the operation, he vomited a quantity of ropy phlegm, mixed with frothy saliva. He continued to struggle violently for some time, then fell quiet for a few minutes, and expired about a quarter before five o'clock.

During the rapid progress of the disease, no source of infection occurred to the recollection of the patient. It was, however, immediately after his death, remembered by several of his comrades, and particularly by two of them, corporals Henry and Moore, of the same company, that a small dog (which was destroyed as mad about three weeks ago, and which had previously bit two other men of the regiment) was in the habit of licking a smallsore on his inner ankle, which is hardly yet cicatrized. The animal was encouraged in this practice by the unfortunate man, under the impression of its being useful to the sore.

The appearances on dissection, about four hours after death, did not differ materially from what has been observed in former cases. The posterior part of the fauces exhibited marks of inflammation, and the papillæ, at the root of the tongue, were uncommonly prominent. The œsophagus was laid open through its whole extent, and in several places shewed slight marks of inflammation. These marks became more conspicuous towards its termination in the cardia. The inner surface of the stomach was in several places inflamed, and in two or three small spots its inner coat abraded. Nothing was contained in it but a small quantity of phlegm. The trachea was also laid open, and in the interstices of the cartilaginous rings exhibited a slight inflammatory redness. The heart was quite sound, as were all the abdominal viscera, with the exception of the stomach; the blood taken from his arm exhibits no appearance whatever of the inflammatory crust, and what was last drawn appears unusually dark coloured.

On the foregoing case, I have only to observe, that, with regard to the nature of the disease, no shadow of doubt can exist. Of seven medical gentlemen who saw the patient, four of us had previously seen cases of hydrophobia; and I considered myself fortunate in having such a body of respectable evidence to adduce. The disease clearly commenced on the morning of the 22d, but when it is considered, that the patient attended a garrison parade that morning, to witness the execution of some sepoys for mutiny; that he performed his duty as orderly sergeant of the company for the day; and although, previous to evening parade, his sensations, at the approach of water, were such as to induce him to have it removed, still he did not think of applying for assistance until the morning of the 23d; when these circumstances are considered, I think it will be admitted that in this case blood-letting had a fair and timely trial. With regard to the extent of the first evacuation, it is to be remarked that, although it was not carried the length of producing actual syncope, the bleeding was performed in presence of the surgeon of the regiment and his two assistants, who although, I believe, in the habit of abstracting blood as freely as most practitioners in this country, could not reconcile it to themselves to carry it farther in this instance. Upon the whole, although I have no wish, on the authority of a single case, to condemn a practice which has raised such high expectations, I cannot help thinking, that a remedy for this formidable disease is still to be considered amongst the desiderata in medicine.

VII.

An Account of Two Trials for Child-Murder.

Yesterday two women, charged with child-murder, were tried at our Castle.

Sarah Hill was privately delivered of a child, which was found in a cellar, covered with sand, wrapped up in an apron, the string of which was tightly twisted round its neck. The infant was examined by a surgeon, on the day of its birth. He found a dark-coloured mark round its neck; the vessels of the head were turgid; some skin was abraded near the right ear; the lungs were quite free from putrefaction, much distended with air, and floated in water. On these grounds, and on the admission of the prisoner that the child had sobbed two or three times, the surgeon stated it to be his decided opinion that the child had been born alive, and had died in consequence of strangulation by the apron string.

The prisoner stated that she was taken ill in her bowels early in the morning. That she got up and went out. On her returning to the house she felt a violent pain, which was very soon succeeded by another, and another. These were so severe as to compel her to seize a loon with her only hand, for she had lost one hand by amputation. Whilst thus supporting herself, the child and placenta fell upon the floor; but she was so ill as to be unable to move until the child was dead. She then fetched the apron down stairs, folded the child within it, and laid it in the cellar, where it was soon found. She admitted that the child sobbed a little, but said that it was born before its time, and also pleaded that she had born many other bastard children, but had not laid violent hands on any of them.

The judge, (Mr Sergeant Marshall), commenting on the floating of the lungs in water, said that this was a disputed test, which had excited difference of opinion amongst medical men, but that he could give the jury no information respecting it. The counsel for the prosecution stated this to be a sign of the child's life, which had been exploded, but lately revived. The jury, after some hesitation, acquitted the prisoner, being perhaps much influenced by the importance which the judge attached to the possibility that the child was premature. The contrary I believe might have been proved, but it was overlooked.

Dr Parr has stated in his Medical Dictionary, that the English courts reject evidence deduced from the experiment of floating the lungs, but this does not appear to be the case; and since putrefaction appears to be the only cause by which it has been presumed that this test can be rendered fallacious, the conclusion of the medical gentleman, with regard to the child's being born alive, seems to have been founded on very fair reasoning. He was not acquainted with the test of Ploucquet, mentioned by Mahon, the translation of whose essay on infanticide was reviewed in your last number.

One morning in the beginning of March last, Mary Eastwood was seen running along, having something wrapped in her apron or petticoat; the crying of an infant was heard at the same time. Pursuit was made, but she eluded it. On the same morning, a man, engaged in husbandry, observed a bloody appearance on the ground, which was slightly covered with snow. He traced footsteps, and occasional marks of blood, to a wood, where he observed that the snow had been removed from some grass near the foot of a larch tree. Separating the grass with his spade, he discovered a child alive, naked, and apparently much starved. The man was soon joined by his master, who, however, did not observe much injury to have been done to the child by its exposure. It was speedily wrapped in flannel, carried to a neighbouring house, and thence, after some hours, to the work-house.

The mother was traced from the tree to a cottage, and was sent on the same day to the same work-house, where she was entrusted with the care of her infant, who was baptized *Moses*. The inhabitants of the work-house described little Moses as looking very red when he arrived, but nothing more was remarked until the following afternoon, when a woman in the room with the mother and infant, heard *a dreadful shriek, like that of a child in fits.* Mary Eastwood being asked if this shriek proceeded from her infant, replied in the affirmative. Half an hour afterwards, the mistress of the work-house found Moses dead in his mother's arms, "*quite black everywhere, except his hands, which were yellow.*"

Two surgeons were sent by the coroner to examine the dead body, but made no dissection, believing, from the account given by the inmates of the workhouse, and from its appearance, that the infant had "*died of a fit.*" One of these surgeons, being questioned by the judge, repeated this testimony, adding, that the appearances would be the same had the child been suffocated by its mother. (This charge formed one count in the indictment.) The learned judge, however, wished for additional information, and I was called into the witness-box, happening to be in court at the time.

I stated, in answer to several questions put by the court, and by council, that the infant, had it been long exposed to the cold and snow, would have fallen into sleep, insensibility, and death; that these primary effects of cold being prevented, a reaction of the system would take place, blood would be determined to those parts which had been previously influenced by the cold; and, if the cold had been severe, inflammation would ensue, and the consequent pain might be so violent as to prove fatal to an infant before mortification could take place. If the child died of such inflammation, it would most probably become convulsed before death. I did believe that the infant in question had been convulsed, at least once. Convulsions might be caused by irritation in the bowels; but convulsions from such a cause would hardly prove fatal at the first attack; neither could they account for the very general discoloration of the child,—at least one fit would not be likely to produce such a discoloration. I did not think the child had been suffocated by its mother, nor ascribed the blackness to any violence used by her, because it was described to have been almost universal. The appearances as described, were such as might be expected, if the child died in consequence of inflammation, produced by exposure to cold.

This woman also was acquitted of the murder, but was convicted of concealment, and sentenced to two years imprisonment in the house of correction In the case of Sarah Hill, the judge did not think the concealment criminal; he hinted that it seemed rather a laudable deference to public opinion.

When it is remembered that both of these unhappy women had denied and attempted to conceal their pregnancy; that both of them had been previously delivered of many children; that both could have obtained instant female assistance, had they wished for it, but that both got as far as they could out of the way of such assistance, we can only draw one conclusion as to the intention betrayed by their conduct. Every humane man must dread to consign any individual to capital punishment, and judges as well as jurymen gladly seize upon any reed that may support a merciful verdict. Yet mercy is also due to the hundreds of infants annually slaughtered in these kingdoms.

I beg leave to request, through the medium of your Journal, answers to the following queries.

1st, Is the floating of the lungs of a child recently born, and free from putrefaction, a sufficient proof that it has respired?

2d, Have any conclusions, drawn from the test of Ploucquet, been given in evidence before British Courts of Justice?

3d, Is the blood of the fœtus which has never respired, incapable of acquiring a florid colour by exposure to the air?

4th, Is there any instance of a still-born child's assuming a florid colour by such exposure? *

Lancaster, *September* 10, 1814.

VIII.

Case of Deafness successfully treated, by the Removal of a Splinter of Wood from the Bottom of the Meatus externus. By John Stevenson, Esq. Surgeon, Oculist, and Aurist, to Her Royal Highness the Princess of Wales, and Lecturer on the Anatomy, Physiology, and Diseases of the Eye and Ear, &c.

Lieutenant-Colonel Webbie Smith, (by whose desire the case is published with his name) of the Royal Horse Artillery, Woolwich, consulted me, on the 25th of last July, respecting a troublesome discharge from his right ear, accompanied with an almost total loss of hearing.

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* To the first of these questions we have no hesitation in replying in the affirmative, and to the second, in the negative, so far as we know. To the third and fourth we request answers from gentlemen who have practical opportunities of elucidating them. We also take this opportunity of mentioning, that the particulars of trials, whose results depend, in a great measure, on medical evidence, will always be highly acceptable; and we wish our correspondents to point out, as accurately as possible, the objects which the Council and Court seemed to have in view in their examination of the witnesses, as well as any observations by the judge on the medical evidence.—*Editor.*

The following particulars were communicated in reply to my inquiries into the origin of his complaint. A spirited horse, upon which he was riding when on duty near Bayonne, the beginning of May, took fright, and, springing from the middle to one side of the road, forced his rider with great violence against some shortened branches of an oak tree, from one of which he received a most severe blow on the right side of his head. The concussion, for a while, so nearly deprived him of his senses, that it was with extreme difficulty he maintained his seat on the saddle. Immediately on recovering himself, he felt a very acute pain in the injured ear. He hastened to camp, and sent for medical assistance. The staff-surgeons found the integuments of the auricle considerably bruised and lacerated, and the concha and meatus externus filled with coagulated blood. The latter was removed by syringing with warm water, and the wound dressed. The local pain and irritation, instead of subsiding, as was expected, rapidly increased, and soon induced a high degree of sympathetic fever, with slight delirium. These symptoms were, in a few days, alleviated, principally by the formation and subsequent escape of a considerable quantity of pus from the meatus internus. After this period, scarcely any uneasiness remained, except on passing the tragus, which was invariably followed by a very pungent and deep-seated sensation. This, together with the deafness and discharge, were regarded as the mere consequence of the preceding inflammation; for which the ear was regularly washed with warm milk and water, and various injections every night and morning. On the arrival of the patient in this country, he took the opinion of several eminent practitioners, and was subsequently recommended to me. An attentive inspection of the organ enabled me to discover something projecting from an accumulation of matter at the farther extremity of the passage, which, by the introduction of a probe, was ascertained to be a solid and slightly moveable substance. The Colonel instantly suggested the probability of its being a portion of fractured and detached bone;—a suspicion he had constantly hinted to his different medical attendants. It was extracted with some difficulty; and, when cleared from adhering discharge, it proved to be a rough angular flat splinter of oak, five lines in length, and three in breadth; one extremity of which, being pointed, had penetrated anteriorly, and in an oblique direction, to the depth of nearly two lines between the cuticular lining and the parietes of the bony canal, close to the membrana tympani.

Size and shape of the splinter.

The presence of this hard foreign body, by mechanically obstructing the auditory passage, accounts for the greatly impaired function of the organ; and its situation satisfactorily explains the source of the discharge, and the cause of the pain arising from pressure upon the tragus. Its removal afforded immediate relief from pain, with the *most perfect* restoration of the sense of hearing; the ulcer also, from this time, rapidly healed, and all discharge ceased.

The most remarkable circumstance relative to this very interesting case, is, that greater and more permanent mischief did not result from the retention of a rough irregular fragment of wood, in the farther extremity of the meatus externus, close to the membrana tympani, for the space of nearly a *quarter* of an *year!* Several instances are on record, and a few similar ones have occurred in my own private practice, in which extraneous substances introduced into the tube, have given rise to the most distressing symptoms, which have continued as long as the foreign body has remained impacted in the canal.

Among others of this description, I may quote the history related by Hildanus, (4th observation of 4th cent.) of a girl 12 years of age, who accidentally let a glass ball drop into her left ear. Such was the succeeding inflammation, and so violent the pain, that the whole half of the head, and even the extremities of the same side became affected. To these were added other alarming symptoms, which continued for *eight* years, and only subsided by the removal of the offending cause. In the 5th observation of Bartholine's Journal, is the statement of his wife's case. She is represented to have been for a long time tormented with a pain round the ear, which was at last remedied by small stones spontaneously discharged from the auditory passage.

The object of this communication is to impress, on the minds of your numerous readers, the necessity of minutely investigating this delicate organ, whenever the diseases, to which it is so very liable, do not yield to common remedies.

Great Russel Street, Bloomsbury, Nov. 19, 1814.

IX.

A Reply to a Letter on the Treatment of Fever by Blood-letting, addressed by the four Permanent Physicians to the Committee of the Fever-Hospital, Cork-Street. By THOMAS MILLS, M. D. &c. &c. &c. *

A LETTER has been published on the treatment of fever by blood-letting, signed by Drs Barker, Stoker, Hagan, and Gamble.

The object of this letter may be considered as twofold,

1st, To shew the inaccuracy of statements, contained in a work of mine, entitled, " An Essay on the utility of Blood-letting in Fever." *2dly*, On such inaccuracy to condemn the practice.

As the authors of the letter have made no trial of this remedy, as they have not watched its effects, † nor examined the appearances of the blood, or the appearances on dissection, they have had recourse to the diet-tables of the hospital. They say, " the data on which they depend have been furnished from the most authentic sources, namely, the diet-tables." (*See Letter, p.* 9.) From these a table has been made out, which is calculated to promote a belief that the sedative plan of treatment was less successful than the usual routine of practice. Two questions now arise:

Are these documents perfect?

Are the statements made from them correct?

If the documents be perfect, and the statements correct, I shall confess that, in some of my calculations, I have erred; but, if it now be proved that the tables are imperfect, and that the statements thence made are incorrect, I hope the four authors of the letter will have the candour to acknowledge that their censure of the practice was not the result of observation or experiment, but of a hasty miscalculation.

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* A due regard to impartiality, and the importance of the subject, induce us to comply with Dr Mills' request, to have his *Reply* inserted in the same work which admitted the *Letter.—Editor.*

† Has Dr Stoker seen a single instance of the practice? I prevailed on Drs Gamble, Hagan, and Barker to look at a few cases; may I beg leave to ask these gentlemen if they saw the same case a second time, and whether a variety of patients ought not to be visited, and visited oftener than once, in order to form a just estimate of the merits of a remedy?

The Table begins thus:

1810.	*Dr Mills' Patients.*		DIED.	
	Males.	Females.	Males.	Females.
From July 14th to 28th, -	22	38	9	3
Counter-statement, -	18	49	4	2

Here are four errors:

1. The addition of 4 male patients
2. The omission of 11 female do.
3. The addition of 5 male deaths,
4. Do. of 1 female do.

See dates and names in the Appendix, p. 1 and 2.

The authors of the letter begin their table, 14th July 1810, yet they include the deaths which occurred among my patients from the 7th to the 14th, while they take no notice of the recoveries that took place during that period; was this correct or ingenuous?

Of the six patients that died, but one was blooded, William Richards, and he died of scirrhous liver, long after fever had subsided.

The errors now pointed out are of different kinds; some of them will be considered, perhaps, rather strange, on the part of gentlemen, who have come forward as the unwilling detectors of error and misrepresentation.—*(See Letter, p. 11.)*

1810.	*Table continued.*		DIED.	
	Males.	Females.	Males.	Females.
From July 28th to Aug. 25th,	32	26	3	3
Counter-statement, -	30	52	3	2

Here are three errors:

1. The addition of 2 males,
2. The omission of 26 females,
3. The addition of 1 death,

See dates and names in Appendix, p. 2 and 3.

Of the five patients that died, three laboured under fever, and one only, Patrick MacDonnell, was blooded. *

Nancy Crosby, entered in my diet-table as dead, I never saw; it is supposed by Mr Price and the Register that she died before the hour of visiting;—this woman cannot, therefore, be considered as a patient treated by blood-letting.

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* In my essay on fever, I only calculate the patients I blooded: Those who were not blooded, though ever so numerous, cannot make for or against the practice. At this particular period, I was not merely cautious, but fearful of the effects of blood-letting, as the practice was novel, and in the success of it my character was committed.

1810. *Table continued.*

			DIED.	
	Males.	Females.	Males.	Females.
From Aug. 25th to Sept. 29th,	15	56	0	3
Counter-statement,	5	32	0	3

Here are two errors:

1. The addition of 10 males, } *See dates and names in*
2. Do. of 24 females, } *the Appendix, p. 4.*

Of the three that died, two had fever, and one was blooded, named Cath. Toole.

1810. *Table continued.*

			DIED.	
	Males.	Females.	Males.	Females.
From Sept. 29th to Oct. 20th,	4	6	0	0
Counter-statement,	0	2	0	0

Here are two errors:

1. The addition of 4 males,
2. Do. of 4 females.

The two female patients under my care were Nurse Saxton and Nurse Conolly.

1811. *Table continued.*

			DIED.	
	Males.	Females.	Males.	Females.
From Jan. 1st to 15th, -	22	0	4	0
Counter-statement, -	30	0	2	0

Here are two errors:

1. The omission of 8 patients, } *See names and dates in*
2. The addition of 2 deaths, } *Appendix, p. 4 and 5.*

John Powell was marked dead in the diet-table, the 1st January, some hours before I commenced my attendance.

Luke Savage is marked dead in the diet-table, the 15th January, the day after I discontinued my attendance; he probably came in on that day or the evening before—his name is not in my journal.

Was it fair in the authors of the letter to reckon these two among my patients?

The two patients that died under my care were G. Carey and —— Murray, and neither was blooded, yet the table might lead the reader to suppose, that *all* my patients were blooded; and this too in direct opposition to the statement in the letter, p. 7, which says, that this mode of treatment was practised only with a large majority of my patients. *

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* Independently of the omissions which I now or hereafter may notice in the diet-tables, there were several patients treated by me at different periods, in consequence of the absence of one or more of my colleagues. Thus, in 1810, from the 8th to the 14th of August, forty-two of Dr Barker's patients came under my care.

1812.	*Table continued.*		DIED.	
	Males.	Females.	Males.	Females.
From May 15th to 30th, -	9	0	2	0
Counter-statement, -	15	1	1	0

Here are three errors:

1. The omission of 6 males,
2. Do. of 1 female,
3. The addition of 1 death,

See Appendix, p. 5.

The additional death was that of Joseph Synge; he was not placed under my care, and opposite his name in the diet-table may be seen, in my hand-writing, "not visited by Dr M." With this remark before the four authors of the letter, this man is not only set down as a patient of mine, but is so set down, that it might be inferred he was treated by blood-letting. And the authors of the letter have come forward as impartial medical inquirers—men anxious to investigate the merits of blood-letting.

1812.	*Table continued.*		DIED.	
	Males.	Females.	Males.	Females.
From May 30th to June 27th,	25	0	1	0
Counter-statement, -	68	13	2	0

Here are two errors of great magnitude:

1. The omission of 43 males,
2. Do. of 13 females,

See Appendix, p. 5. and 6.

But there is a third error, and one of the most unusual kind, viz.—the omission of the death of a patient, named J. Raper, who died apoplectic 19th June, in the convalescent wing. This omission, and the omission of so many of my patients, may be ascribed to the loss of part of a diet-table, including patients from the 30th May to 18th June. This chasm I have filled up from my journal.

The authors of the letter have stated that "the data on which they depend have been furnished from the most authentic sources, namely the diet-tables."*

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* Were the diet-tables neither defaced nor imperfect, can they be regarded as medical records, furnishing the data necessary for investigating the merits of any mode of treatment? The same diet-tables, and the same set of patients, from change of wards and change of duty, and from the occasional absence or indisposition of the attending Physician, have come under the care of two or more of his colleagues. On three different occasions I had reason to complain of an ungracious interference with my practice. I was removed from my wards without notification or apology, and thus the management of patients was taken out of my hands, with whom a plan had been pursued, in the success of which, for obvious reasons, I was deeply interested.

They have likewise stated, that *all* the patients treated by Dr M. are given in the table, yet here we find 56 omitted.—(*See Letter*, p. 7.)

1812. *Table continued.*			DIED.	
	Males.	Females.	Males.	Females.
From June 27th to July 25th,	70	0	3	0
Counter-statement, -	84	11	2	0

Here are three errors:

1. The omission of 14 males,
2. Do. of 11 females,
3. One additional death,

See Appendix, p. 6, 7, and 8.

J. Dowling marked dead in the diet-table, 6th July, was not a patient of mine—it is supposed he died before the visiting hour.

During the above period 95 patients were placed under my care, two died; this makes the deaths to the recoveries not 1 in 47, yet it has been stated by the authors of the letter that at no period did I lose so small a proportion as a twenty-fifth.—(*See Letter*, *p.* 10.)

1812. *Table continued.*			DIED.	
	Males.	Females.	Males.	Females.
From July 25th to August 29th	70	31	6	0
Counter-statement - -	58	38	5	0

Here are three errors:

1. The addition of 12 males,
2. The omission of 7 females,
3. One additional death.

See Appendix, *p* 8 and 9.

August 5th.—John Dunn, one of the five persons that died, is marked thus in the diet-table: "died apparently of hydrocephalus internus the day after admission," no remedy was taken.

August 21st.—Martin Bryan is marked thus: "not seen by Dr M."

No notice has been taken of these remarks by the authors of the table.

1812. *Table continued.*			DIED.	
	Males.	Females.	Males	Females.
From August 29th to Sept 26th,	24	19	2	2
Counter-statement, - -	12	33	2	2

Here are two errors:

1. The addition of 12 males,
2. The omission of 14 females,

See Appendix, *p*. 9 and 10.

T. Laurence, one of the patients that died, was not blooded—he took no medicine.

A part of a diet table is here lost, containing the names of patients from the 2d to the 23d September. It has been stated by the authors of the letter, "that the data on which they depend have been furnished from the most authentic sources, namely, the diet-tables."—*(See Letter.)*

1812.	*Table continued.*		DIED.	
	Males.	Females.	Males.	Females.
From Sept. 26th to Oct. 31st,	58	21	3	2
Counter-statement, - -	112	15	2	2

Here are three errors, the first of which is of great magnitude:

1. The omission of 54 males,
2. The addition of 6 females,
3. One additional death.

} *See Appendix*, p. 10, 11 & 12.

Anne Doherty died between the 8th and 16th of October; although I was absent from the Hospital during this period, yet I have included her among those who died under my care.

W. Barry, Sept 26, is marked thus in the diet-table: "died a few hours after admission, of mortification in the leg."

In page 7 of the letter it is asserted, that *all* the patients treated by Dr M. are given in the table; here, however, we find an omission of no less a number than 48.

1812.	*Table continued.*		DIED.	
	Males.	Females.	Males.	Females.
From Oct. 31 to Nov. 28 -	83	0	5	0
Counter-statement -	76	0	4	0

Here are two errors:

1. The addition of 7 males,
2. An additional death.

} *See Appendix*, p. 12 & 13.

Of the four patients that died, two were blooded—one of the four is entered in my journal as "the dying apoplectic yellow fever patient." I have reckoned him as a patient, but as he was not blooded, he cannot be included among those treated by blood-letting.

Roger O'Brien is marked in the diet-table as dead the 28th November—the statement only goes to this date, and this death I include in the following period.

1812.	*Table continued.*		DIED.	
	Males.	Females.	Males.	Females.
From Nov. 28 to Dec. 31 -	78	0	4	0
Counter-statement -	88	1	4	0

Here are two errors:

1. The omission of 10 males,
2. Do. 1 female.

} *See Appendix*, p. 13 & 14.

Of the four that died two were blooded. The diet-table from the 12th to the 31st December is lost.

From such defective documents as these diet-tables, the four gentlemen undertake to decry the practice of blood-letting in fever, and that in the face of a statement which they do not attempt to contradict, by which it appears that in 1810 blood-letting was employed in 209 patients, and in 1812 in 504; that during the first of these periods the deaths were to the recoveries in the proportion of 1 to 23, and during the second as 1 to 28 only. Suppose, however, these documents perfect, they could only prove that a certain number of my patients died who were not blooded, but, from this concession, no inference could be drawn unfavourable to the practice of bleeding in fever.

The authors of the letter admit that the recovery of my patients was more rapid than that of others, but are unwilling to allow that it was owing to the mode of treatment; they say "most probably it arose from Dr M. having under his care a much larger proportion of *male* patients than the other physicians." Here probability is opposed to fact, and a fact of their own stating, that fever is more violent in males than in females; Now is not recovery slow in proportion to the violence of the attack? And does it not hence follow, that the more speedy recovery of my patients was owing to the practice adopted? Here I may be allowed to remark, that had the majority of the patients whom I bled been females instead of males, it is fair to presume that still fewer cases would have terminated fatally.

In page 9 of the letter, we find the following passage: "it does not appear from any other inquiry, that Dr Mills's patients were less liable to relapse than others, and in many instances they were observed to remain in the hospital in such states of debility, as are usually consequent to the repeated detractions of blood." This assertion is unsupported by fact, and appears to be a contradiction of a previous statement in the letter, that my patients remained a shorter time in the hospital than those of the other physicians, which necessarily goes to shew that the strength of my patients was preserved by the practice: Some instances, indeed, of lingering convalescence might have occurred among those who recovered in consequence of bleeding, and who had laboured under symptoms, which, treated after the usual manner, would have proved fatal.

From the above remarks as to the tardy recovery of my patients, we are led to suppose that the authors of the letter had not only watched the immediate effects of the remedy, but had traced it to its remote consequences; whereas, I am sorry to say, this was far from being the case. In my work on fever, no attack was made or intended to be made on any of my colleagues—I never presumed to censure them for not approving of the practice which I had adopted; but I shall always lament, for the sake of humanity and science, that they did not attentively examine its effects; effects which I have invited both them and the faculty to witness.—Would I have ventured to do this had my practice been unsuccessful? Would the patients have called for a repetition of the remedy if it had not afforded relief; or would the nurse-tenders have been blooded at their own desire, without the advice of a physician! But, above all, can it be supposed that the watchful guardians of the Institution would have allowed the continuance of a mode of treatment, novel, condemned by whisper, not adopted by any of my colleagues, if it had not proved beneficial? and it will not be said that the Committee were ignorant of its effects, or of any of the transactions of the hospital—this valuable and intelligent body of men had not recourse to defective diet-tables; they satisfied their minds of the safety and utility of the practice from sources far more authentic.

[In an appendix is a list of the names of the patients, with the dates of their admission, and death when the event was fatal; and the whole is concluded by the subjoined note.]*

X.

Letter to the Editors of the Edinburgh Medical and Surgical Journal, from the Medical Officers of Greenwich Infirmary, vindicating Sir William Adams from the charge of keeping secret his Improved Operations on the Eye; with some Observations by the Editor.

GENTLEMEN—A statement having appeared in the last number of your valuable journal, charging Sir William Adams with keeping secret his improved operations on the eye, we consider it but an act of justice to the individual in question to state, that his uniformly open and liberal conduct towards us, since his appointment as operator for diseases of the eye to the Royal Hospital, has been such as to merit our warmest esteem and respect: That, so far from his manifesting any disposition to operate in private, we have been, and still continue, by his permission, in the constant habit of inviting our medical friends to witness all operations performed upon the pensioners placed under his care; and also, that we have generally observed, on these occasions, numbers of the profession in private practice, present, invited by Sir William Adams himself, to whom he has, in our presence, explained the nature of the different operations they had seen him perform, with the same minuteness and candour, which, in every instance, we have ourselves experienced from him. Not doubting you will give the earliest publicity to the above communication,

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* NOTE. The Names now given are chiefly extracted from the Diet-tables of the Hospital; where these tables are defective the chasm is filled up from my Journal, in which the names of my patients and the remedies prescribed were daily registered. To Mr Price, William-street, I beg leave to return my thanks, not only for the assistance he afforded me in making out the present list, but in forming the tables given in my Work on Fever. Mr Price acted as Apothecary to the Hospital during a great part of the year 1812, in consequence of the indisposition of Mr Nicholls and of Mr Blair.

We are, gentlemen, your obedient servants,
(Signed) R. Robertson.
B. M'Laughlin.
M. S. Kent.

Royal Infirmary, Greenwich, August 29, 1814.

Observations by the Editor.

We sincerely rejoice that we were mistaken in the opinion we had formed of Sir William Adams. But while we do justice to his character, we must vindicate our own. With Sir William Adams we have no personal acquaintance; we formed our opinion of him entirely from his writings. We have again perused the "Official Papers" with the greatest attention, and we are convinced that every impartial person, judging from them, would form nearly the same opinion that we did. The whole publication still seems to us solely calculated to serve the private interests of Sir William Adams; for it conveys no other information to the public, than that he is the most scientific and successful of all oculists; and has invented peculiar and appropriate instruments and modes of operation, for the cure of some of the worst diseases of the eye. We find, indeed, that he personally explained to the Board the nature and effects of his several operations; and that he communicated to the medical officers much valuable information and instruction relative to diseases of the eyes. But, in this very widely circulated pamphlet, there is not only no communication to the profession at large of the nature of any of his discoveries; but, from several passages, we very naturally concluded that no early communication of the kind was intended. We appeal confidently to the two following paragraphs, one taken from the report of the medical officers, the other from his own letter to the Board.

"In addition to the gratifying contents of the second report, we think it our duty to state, for the information of the Board, that Mr Adams has discovered a mode of curing the Egyptian ophthalmia, which has been successfully practised upon several of the pensioners, some of whom had been blind for three or four years, and given up as incurable by the most eminent oculists then in London. The communication that this destructive and hitherto intractable disease admits of cure we conceive will be gladly received by the Board; and the promulgation by Mr Adams of this important discovery be considered as a great *national desideratum*."

"Where the cateract is too hard and solid to admit of this immediate division, I do not attempt, as was my former practice, to effect its absorption by a frequent repetition of the operation; but I at once extract it. This, however, is accomplished by a process *totally different* from that I have felt it a duty to deprecate; a process which I must claim to be novel, and which happily attains the highly important desiderata which had been hitherto considered *unattainable*, while it obviates the many causes of failure which rendered the usual mode of extraction so generally unsuccessful. From the principle upon which it is founded, and the favourable results of its termination during the last two years that I have extensively practised it, I feel myself warranted in asserting, that it possesses the utmost degree of excellence which it is *possible* for extraction to arrive at, and that its general success will prove nearly as great as the operation for the removal of the soft cataract. To deter other persons from claiming it as their invention, or anticipating me in its communication to the public (as was the case with my instruments and operation for the cure of the *soft cataract*, and my successful revival of an obsolete operation for *artificial pupil*), I have requested Mr Maclaughlin to record on the hospital books, the different stages of this operation, as he has seen me perform it on several of the pensioners."

It was expressions such as these which led us to suppose that Sir William Adams meant to preserve secret his discoveries, as *Nostrums*; and we scarcely regret the publication of our suspicions, since it has led to their contradiction from so respectable a quarter.—*Editor.*

XI.

Case of Compound Fracture of Both Legs, successfully Treated.

By J. PEAKE, Member of the Royal College of Surgeons.

IN the night of the 2d of November 1812, I was desired to visit Michael Norris, a man about 36 years of age, who had been ran over by a waggon loaded with coals. Both wheels had passed over the legs, from three to four inches above the ankle joint.

In the right leg I found a very extensive fracture, and the wheels here, it would seem, had passed in a direction to break a portion of the tibia, from three to four inches in length, in two places. This portion of bone could be felt under the integuments, detached at both ends, and, indeed, to the extent of it, the leg seemed to be dreadfully crushed and bruised.

In the left leg, both bones had been fractured about the same place, and protruded considerably through a wound upon the inner side.

So soon as I could get my patient to bed, I endeavoured to replace the bones as well as the nature of the case would allow. In the left leg, where the bones had protruded, I had no very great difficulty to reduce the fracture, and to bring the ends of the bones into apposition, without dilating the wound. In the right leg, also, I replaced the fractured portions of bone, as nearly as I could. The difficulty I had now to contend with, was to put the limbs into such a position as would be favourable to their reunion; and from the very extensive nature of the accident, one alone could be chosen, and that was upon the heel, properly supported by pillows, &c. &c.

The inflammation and fever resulting was, as may be expected, truly alarming. On the first day, he complained very much of pain; the pulse becoming hard and full, with great thirst and anxiety, and in the night delirium succeeded. Twenty ounces of blood were directed to be taken from the arm, and the antiphlogistic plan and regimen to be strictly attended to; the venesection to be repeated whenever symptoms might indicate its necessity, and to the fractures themselves were directed such applications as were best suited:—in the one leg, to promote a speedy union of the wound, and to the other, such as would best subdue the attendant inflammation.

For the first week, these symptoms continued; the inflammation and swelling in the right leg particularly extending up its whole course, and also up the thigh, attended with most severe pain at the fractured part, where, after a few days, suppuration took place, and matter made its way out from a wound, directly opposite to the superior end of the fractured portion of tibia already described. The next thing that presented itself, as may be expected, as the tension and swelling of the limb subsided, was the end of this piece of bone, denuded of its periosteum, pointing through the wound, nearly to the extent of an inch.

The part from which it was broken below, could easily be felt and distinguished; a line could also be perceived by the touch, marking its separation. It seemed firmly attached to the integuments, beyond the part which was denuded of its periosteum. Still I was led to expect that the whole would eventually exfoliate.

The discharge from the wound of this leg was very considerable; and as the symptoms of inflammation and fever subsided, a disposition to hectic fever presented itself; and there was as much difficulty now to support the patient's strength and power, as there had been previously to prevent the fatal effects of the inflammatory stage.

A generous diet, together with decoct. cinchonæ, porter, &c. and an opiate at bed-time, was the plan observed.

From the other leg, also, there was much discharge. The wound healed but slowly, although the bones were kept perfectly in apposition; and from this leg, from time to time, there were exfoliations of small pieces of bone, which, of course, retarded its healing.

Things continued in this state for two months longer; the bone still remaining in the right leg, in the same position, firmly attached to the integuments over it, and appearing now not more likely to exfoliate, than it had been for weeks past; and that part of it which had already made its way out, and had been exposed for so long a time through the wound, still retaining its natural and healthy colour, from which I was led to expect that the bone itself must still be supplied with blood, and that probably a process of union might be going on between the detached parts below.

This was also the opinion of an ingenious friend, who had from time to time rendered me his assistance in the treatment of this most intricate case; and in this we were well confirmed, for, on the 1st of February 1813, three months from the time of the accident, matters still remaining in *statu quo*, we determined to lay bare the fractured portion of bone, throughout its whole length, and were not surprised to find, as we had anticipated, that a process of union had taken place below, and that the bone itself, at that part, was firmly united. We had previously determined, should this prove to be the case, to remove all that portion denuded of its periosteum; an operation both tedious and difficult, but which we readily accomplished, by means of Hey's saw,—an instrument which Mr Hey of the Leeds Infirmary has introduced to public notice in his very valuable surgical book, and with which he has successfully operated in several cases of cancer of the tibia, and also in cases of fractured skull. To this instrument, I think I may now very justly state, my patient owes the preservation of his limb, and very probably of his life. After dissecting away the integuments from the bone, and turning them back, I sawed through the tibia, exactly one inch below the projecting edge of the fractured portion of bone, and after having removed it, I carefully replaced the integuments, leaving more than two-thirds of the fractured bone behind; I then applied adhesive straps, to bring on union of the parts as soon as possible.

The wound healed very rapidly, and the direct improvement in my patient's health was astonishing. The discharge from the wound soon became inconsiderable, and a deposit of ossific matter soon also filled the space left in consequence of the removal of the piece of bone.

In the course of a month he was able to sit up five or six hours every day, and the leg upon which I had operated became even stronger than the other.

From this time he continued progressively improving, and was not only enabled to sit up during the day, but very soon to bear upon his legs with as much firmness as if no accident had occurred.

The stiffness and weakness both of the ankle and knee-joints, resulting from the confined position in which, for so long a time, they had been placed, and during which they had been altogether in a state of disease, was a difficulty to surmount which time alone could effect. This he has now so far recovered, that, for some time past, he has been able to walk with the aid alone of sometimes one and sometimes two sticks. Slight exfoliations of bone have also, from time to time, proved troublesome, and have retarded, in some degree, the progress of cure, but he is now so far well, that he can walk the distance of half a mile from his own house, and I have no doubt but that he will be enabled eventually to resume his former occupation, which has been that of a labourer.

Chipping Ongar, Essex, August 15, 1814.

XII.

Case in which a mass, resembling a Placenta without a Fœtus, was discharged from the Womb. By M. Lemon, Member of the Royal College of Surgeons in London.

The following case being considered very important, I have been advised by many eminent professional gentlemen to give it publicity. With diffidence, therefore, I submit it to your consideration, and beg you will insert it in the next number of your valuable Journal.

Mrs Cunningham, æt. 24, has had three children, besides a miscarriage, which happened about two years ago. The extreme rudeness of a licentious fellow having, on Thursday the 11th of August, required her to make violent exertion, she, on the following day, perceived a slight flow of blood from the vagina, which ceased on Saturday evening. On Sunday night, when in bed, it returned with increase, and continued, (gradually diminishing) till Wednesday. All this time she suffered no pain, nor was she prevented from managing her domestic affairs:—There was a slight tenderness of the abdomen only. After having had two or three pains, in every respect like those experienced during the time of parturition, she, about half past five in the evening, was seized with a profuse hæmorrhage. The pains and hæmorrhage continuing, my immediate attendance, for the first time, was requested. It was half past nine when I saw the patient, who told me she had advanced near four months in her pregnancy; and that the discharge was alarming only during the presence of a pain. Examining *per vaginam*, I found the *os uteri* dilated to the extent of a half-crown: and a bag, protruding through it, distended, as I thought from its elastic feel, with the liquor amnii. About eleven o'clock, a fleshy cake, three inches in diameter, possessing every character of a natural placenta, and having a membranous bag connected with it, was expelled. The shape of this mass was oblong. Slitting up the bag,* which was then flaccid, the contents gave an appearance (not with regard to colour) similar to that which is presented on the exposure of the thoracic and abdominal viscera of a very young fœtus. But the expansion of the placenta rendered the nature of this appearance evident. Its whole surface was covered with tumours. There were about twenty-two distinct, besides many inconsiderable ones, of various size, shape, and colour; some in clusters, (with broad bases) all seemingly connected by veins. The largest tumour was equal in magnitude to a small walnut; but this was easily, though not completely, divisible into two. Many were of the size of grapes; some were flat-sided; others conical, oblong, irregular, and convoluted. Most were livid; some fleshy-brown, and two or three light yellow. There was one, larger than the rest, semitransparent, membranous, and globular, with its base surrounding the largest of the latter.* This appeared to be formed by the raising of the covering of the fœtal surface of the placenta, which, after affording a tunic to all the tumours, was continued from the circumference of the placenta to form the bag. This membranous continuation had another imperfect fatty layer proceeding from the uterine surface of the placenta. The livid tumours generally had condensed fat at the extremity. All had the same feel, except the yellow, which consisted of a compact fatty substance, resembling that found on the other side of the placenta. The livid and brownish had not the firmness of fleshy tumours, nor the elasticity of those containing fluid. They contained coagulated blood; and, pressing it out, a substance of a placental nature was left betwixt the fingers. Such is my idea of these tumours, that, were my opinion of the formation of them demanded, I should say it was probable that the blood, being extravasated from the vessels of the placenta, or having accumulated in its cells, that tuberculated surface was produced by an irregular and great distention, or, perhaps, rupture of them. Notwithstanding, I presumed it was evident that some of the veins, or sinuses of the placenta, had degenerated into these tumours, as their regular progression could apparently be traced.

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* There was no fluid found when the bag was cut into.

The woman, during the growth of this mass within the uterus, had every symptom of pregnancy. The breasts, when she is not pregnant, are scarcely observable; now, they are rather prominent, and distinctly marked. About the third month of gestation, a soreness was always produced by pressure; this she felt at this time, under the same circumstances. She had the same feelings,—the same nausea,—and the same capricious appetite, she always had when with child. There was a prominent firmness of the belly, and a total disappearance of the menses. At the time she was taken ill, she expected to quicken; but she yet had not felt any internal motion.

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* I cannot say whether this vesicle contained air or water, as it was relaxed before I could attend to it.

Here are the most satisfactory symptoms that accompany a natural and common conception, yet not even the rudiments of a child have, by the strictest examination, been discovered. The bag in which the child, if it had existed, would have been found, was, as far as I could judge from a particular inspection, perfectly entire. It therefore cannot be consistently conjectured that the child might have escaped at the time of the flooding.

That the womb had not contained a fœtus, even just before the threatening of the supposed abortion, is proved beyond doubt, by there not being the remains of a funis, or by the mark of its attachment not existing. Nature, very likely, intended to have created a child, but whether the embryo had been formed, and afterwards dissolved, I do not know; nor indeed have I at this moment leisure to prosecute the inquiry with sufficient steadiness: I shall, therefore, leave it to the ingenuity of others.

The 9th plate in Denman's 4to edition, published in 1805, gives a pretty correct representation of the body I have endeavoured to describe.

I have searched Morgagni, and many other celebrated authors, with some degree of diligence; yet not one case have I been able to find exactly correspondent to this. Every case that throws light upon medical jurisprudence, must, by every well-wisher of mankind, be considered highly important, not only in a judicial, but in a moral and political point of view. Our present knowledge of it is very limited; our ideas unsettled, and indefinable. The works written professedly upon this subject, are either too confined in their plan, or too speculative in their execution. Cautious deductions drawn from absolute facts, or facts themselves, should constitute a practical system of medical jurisprudence.

Those characters by which we are taught to distinguish a uterus lately impregnated, are, a ruggedness of a certain extent, according to the time of gestation, of its internal surface, —the size of its cavity,—the thickness of its walls,—and the largeness of its mouth. But, upon the first of these, the greatest stress is laid. I seriously ask you, then, would not the womb of my patient have exhibited the same characters? Now, sir, had this woman died from hæmorrhage, and had her death been made a subject of legal investigation;—and had I been desired to examine the body, without having any previous knowledge of the person,—what evidence must I have given?

Had I doomed an innocent fellow-creature to eternal infamy by rashly swearing that a child had been recently expelled from the womb,—from the placental mark,—from the capacity of the cavity of the uterus,—and from the dilatation of its mouth, I should have been supported by gentlemen who are justly regarded as some of the brightest ornaments in the medical world. The mark of a placenta has, by very eminent practitioners, been insisted on as "an indubitable proof of the existence of a child." I most sincerely wish, for the good of society, it could be proved to be so. I am not ashamed to confess that I am totally unacquainted with any circumstance, which, with certainty, points out the difference in this womb, from a womb having contained a fœtus. But perhaps the kindness of some of your correspondents will favour me with information on this subject, which will much oblige, &c.

Liverpool, Nov. 19, 1814.

Observations on the preceding case.

We agree with our ingenious correspondent in considering this case as of very great difficulty and importance, but, as the appearances occurred in a married woman, we think it much more likely that they were really the effect of impregnation, than that the whole phenomena of conception, the cessation of the menstrual discharge, the swelling of the breasts, the nausea and capricious appetite should arise, in their natural order, from any other cause. The only circumstance against this opinion is, the total absence of a fœtus, or umbilical cord. But this furnishes no conclusive argument; as there are innumerable instances of fœtuses so exceedingly imperfect, that their nature can scarcely be recognized; and, with a still more imperfect organization, they degenerate into a molar mass like the present, for it is not without example. Voigtel, in his excellent manual of pathological anatomy, thus expresses himself. "True moles are distinguished from the false, and other growths in the uterus, by their not deriving their origin from the substance of the womb or its membrane, but by their being always the consequence of conception; while in others, from an originally imperfect developement of the ovum, or an injury to the fœtus at its first formation, it appears either as a shapeless mass, or *the fœtus itself is completely destroyed, and only its membranes and the placenta continue to grow for a time,* and get thickened and fleshy, or filled with fluid only, or form membranous, fibrous masses, or hydatids, or assume other unnatural appearances." —*Handbuch der pathologischen Anatomie, Vol.* III. *p.* 501.

Entertaining this view of the case, therefore, we should have little hesitation in considering the expulsion of a placenta without a fœtus, as in the present instance,—or the distinct mark of a placenta, as in the case of Miss Burns, as being a conclusive proof of impregnation and conception,—not indeed of the production of an organized fœtus; but, if the object only be to prove preceding sexual connection, it is of no consequence whether the fruit be perfect or not. It is far otherwise when the discharge of such a mass leads to the suspicion of the infant having been murdered and secreted, and it is in this respect that the preceding case appears to us particularly important; for, if Mrs C. had been an unmarried woman, and had expelled this placental mass privately, and it had been soon afterwards found, and carelessly inspected, and she had been suspected, and examined, there would have been every proof of child-murder against her, except the actual finding of the fœtus itself. We therefore see the propriety of preserving and minutely examining the placenta in all such cases, with the view particularly of ascertaining the state of the umbilical cord, and the necessity of producing the body of the murdered infant, or at least directly proving that a living child was born, in order to constitute the crime of child-murder.—*Editor.*

PART II.

CRITICAL ANALYSIS.

I.

Pathological Researches. Essay I. On Malformations of the Human Heart: Illustrated by numerous Cases, and five Plates, containing fourteen Figures; and preceded by some Observations on the method of improving the Diagnostic Part of Medicine. By J. R. Farre, M. D. 8vo. pp. 46. Longman and Co. 1814.

Dr Farre is indefatigable in his pursuit of morbid anatomy, and has commenced, by the publication of this Essay, another series of papers, which is upon a much less expensive scale, and is not intended to interfere with his elegant and interesting work on the diseases of the liver, as we learn from the following passage.

> "Convinced of the utility of this labour, the author is desirous of adding to the common stock, the observations which he has been enabled to make. Before he undertakes to give those which have presented themselves to him on all the internal organs, he wishes to be permitted to communicate, in particular, what respects the heart, the alimentary canal, and the liver, in separate papers, carried on at the same time, for the convenience of referring to signs and morbid appearances, which have a relation to each other. The first number on the liver has already been published, and the second will shortly appear, the plates for it being finished. Instead of publishing the Diseases of the Heart in a series of numbers, it is proposed to give them in distinct essays. He pledges himself to complete what he has undertaken, as far as his health will permit, or the willingness of the medical public to bear a just proportion of the expense of so considerable an undertaking will justify him in doing; for, in performing his professional duties, he must not forget that he has others of a higher nature to fulfil." p. ix.

We trust that this just appeal to the profession will be met with equal justice; for, in that case, we doubt not that we shall be put in possession of the most complete series of the organic changes, which disease produces in the human body, that has ever been accomplished, arranged and illustrated in a manner the best calculated to render it a source of sound instruction. For we are persuaded, that no practitioner has so assiduously pursued the investigation of morbid anatomy with so clear an object, *i. e.* for the sole purpose of connecting morbid changes with symptoms, as Dr Farre, or has obtained a more complete collection of illustrations of these symptoms. We have already, indeed, expressed our opinion, when examining his first essay on the liver, that some modern pathologists are much too sanguine in their expectations of the additional pathological information to be derived from such inquiries: but it is certain, that no practitioner can be too industrious in multiplying his observations, whether of symptoms, or of the seats, causes, and effects, of the maladies which they indicate.

This essay, which contains a statement of, or a reference to, "nearly all the cases of malformation that have been published," will be followed by a second, which will treat of Pericarditis and Carditis, and by a third, in which Chronic Carditis will be the subject of illustration.

In what he calls an advertisement, Dr Farre has candidly stated the difficulties which exist in the way of improving the diagnosis of diseases, and the actual imperfection which is observed in that particular point, even in the opinions of the ablest physicians. This difficulty, he admits, arises from the want of uniformity in the signs, which accompany morbid changes, and from the sympathies between distant organs, by which they are reciprocally disturbed by the disorders of each other: whence not only the diseases of the heart and the brain, the centers of the two great systems, the sanguiferous and the nervous, are often confounded; but still greater errors in diagnosis are committed, even by those who are accounted the best practitioners, in treating some of the diseases of the stomach, the liver, and the heart respectively, for each other. Hence, the author remarks, the physician is often obliged to be satisfied with a conjecture, respecting the seat and nature of the patient's disease.

"Very commonly, in giving his opinion of the case, he will content himself with saying, that his patient labours under some disease of the liver—some disease of the heart—some disease of the brain, &c. This mode of expression is so familiar, that every medical man continually hears it, nay, often uses it. But if words be the signs of ideas, how loose and imperfect must be the conceptions of diseases, which, by the very expression, are thus rudely blended and confounded! What other than an empirical method of treatment can be founded on a diagnosis so purely conjectural!" *Advert.* p. v.

If there be any severity in this remark, it consists in its truth. But we fear the very truth implies, not that physicians have, for two thousand years, omitted altogether the right mode of investigation, but that the subject is incapable of demonstration, or of any degree of evidence approaching to it; that the signs and the maladies are not so invariably connected, as always to imply each others' existence; that, on the contrary, some signs that are prominent in one instance, are in others altogether absent, when even the anatomist cannot mark the difference of morbid change; and that above all, the sympathies, to which the author has alluded, mixing with the realities, will at times delude the most cautious observer. All this, however, admitted, we allow to the author, that, every individual, who cultivates morbid anatomy, will obtain the best assistance in removing and overcoming these difficulties; and that the following advantages, which he has pointed out, may be justly ascribed to this species of investigation:

"*1st*, That this method, preceded by clinical observation, affords the only means" (or at least the best) "of distinguishing between diseases of function and diseases of structure; and the most probable ground for the important opinion respecting the lapse of the former into the latter. *2dly*, That it is capable of introducing great simplicity into the methodical arrangement of diseases, nosologists having exceedingly departed from nature in the multiplication of their species, and consequently of their genera." And, "*3dly*, That by rejecting the artificial forms, and studying only the natural or true species, it enables us to follow, with greater precision, their respective stages, to separate the characteristic from the anomalous signs, which were noted in their histories, and, by thus establishing a more accurate diagnosis, to adapt the treatment not merely to the name, but to the form and stage of the disease." (p. ix.)

The author begins his essay by a short Introduction, in which he exemplifies the value of morbid anatomy, in the information which it has afforded upon this very subject, malformation of the heart. So late as the year 1780, the case of a *blue* girl, in whom the usual symptoms had existed from birth, was described by a French physician, in the Hist. de la Soc. Roy. de Médecine, which he deemed a species of *jaundice*; and two similar instances were mentioned by the same author, as having been treated by another physician as cases of *scurvy*. Such mistakes could not now occur.

"Anatomical observation has now, indeed," says Dr Farre, "enabled us to distinguish, with sufficient precision, the character which belongs to *ordinary* malformations of the heart. It has shewn us farther how to discriminate between it and an *approximation* to the same character, which occurs under a certain condition of the lungs, a point which we shall consider in its proper place. It has also taught us, that, in particular cases of malformed hearts, although black blood should not be so much in excess as to give the characteristic colour of the skin, the malformation may yet be discerned by the peculiarity of the respiration." (p. xiv.)

The study of the varieties of malformation, which take place in the human heart, enables us to discover "several degrees of imperfection in the function of that organ, by which our knowledge of the proper office of the lungs is improved;" it also contributes to throw some light upon the sources of animal temperature. A double heart is essential to the perfection of all warm-blooded animals, in which the whole mass of blood must pass through the lungs, before it is distributed to the other parts of the body. And Dr Farre remarks, that all the imperfections of configuration, to which the structure of such an organ is liable, are productive only of two errors in its functions, (varying in degree according to the extent of malformation); namely, 1. The mingling of black with red blood; and 2. impeding the circulation. This general fact Dr Farre has made out, and adopts it as the ground of classification. He arranges, therefore, all the varieties of malformation that have been recorded, under two species, distinguished by these two imperfections of function. Possibly the following tabular view, which the author has given as a table of contents, may afford the reader the most satisfactory idea of the varieties, which he has described and figured:

Species I. Malformations of the Heart, or of its Arteries, mingling *black* with *red* blood.

Sp. I. *var.* 1. Single Heart.
— I. — 1. *a.* Two Pulmonary branches from the Aorta.
— I. — 1. *b.* One Pulmonary branch from the Aorta.
— I. — 1. *c.* Heart transposed. The Aorta and Pulmonary Artery branching from a common trunk.
— I. — 2. Imperfect Double Heart.
— I. — 2. *a.* Unclosed Foramen Ovale.
— I. — 2. *b.* Dilated Foramen Ovale, or imperfect Septum Auricularum.
— I. — 2. *c.* Dilated Foramen Ovale, with an open Ductus Arteriosus.
— I. — 2. *d.* Ostium Arteriæ Pulmonalis communicating with both Ventricles.

Sp. I. *var.* 2. *e*, Dilated Foramen Ovale, and contracted Ostium Arteriæ Pulmonalis.

— I. — 2. *f*. Dilated Foramen Ovale, with an open Ductus Arteriosus, and impervious Ostium Arteriæ Pulmonalis.

— I. — 2. *g*. Perforation of the Septum Ventriculorum.

— I. — 2. *h*. Ostium Aortæ communicating with both Ventricles.

— I. — 2. *i*. Transposition of the Aorta and Pulmonary Artery.

For the particulars, both of the appearances of the heart and great vessels, and of the symptoms, which were observed during life, we can only refer the reader to the work itself, and to the well executed figures accompanying it. All that we can attempt in analyzing a work, which contains so much detail, is to notice some of the leading observations of the author as we pass on. For though the whole lies in the compass of but forty-six pages, the words might well have been dilated into a volume of 150 pages, in the ordinary mode of printing, and the matter into one of twice that bulk.

In speaking of the unclosed foramen ovale, Dr Farre remarks upon the frequency of such an appearance in the dissection of adult persons, in whom, however, no particular inconvenience appears to arise from it; a circumstance which he thus explains. "In these simple cases the communication is so oblique, that the valvular office must by this very arrangement be still performed, if not perfectly, at least so well as to prevent any perceptible disorder of the vital functions." p. 8. Dr Farre thinks the opinion of the late Mr Allan Burns, that such a state of the heart in old people is accompanied by dyspnœa, is not well founded; and justly expresses his surprise, that the celebrated M. Corvisart should mention two instances of the open state of this foramen, which he had met with, as rare phenomena. If the septum auricularum is imperfect (as in I. 2. *b*) occasional dyspnœa, and more or less permanent blueness of the skin, occur. But if, with a dilated foramen, there is combined an open ductus arteriosus, the vital functions are disturbed in proportion to their extent; blueness, dyspnœa, palpitation, faintness on exertion, obscureness of pulse, &c. occurring in various degrees, as in several cases here quoted and detailed. The variety I. 2. *f*. is more frequent than those which precede it in the table; but the variety I. 2. *h*. which was first described by Sandifort, is, Dr Farre affirms, by much the most frequent, and is pretty constantly combined with one or other of the preceding varieties, many examples of it having been recently recorded and observed.

Having concluded his detail of illustrations, Dr Farre proceeds to the ratio symptomatum. The permanent blue colour of the skin affords a sign that a very large proportion of black mingles with the red blood: but malformation may exist, where this hue is not present. Exceptions to this hue are mentioned in I. 1. *a.* and *c.*, and in I. 2. *d.* From these exceptions, the author remarks,

> "We learn, that if the *full* proportion of blood be circulated through the lungs, although the red blood be subsequently mixed with an equal proportion of black blood in the heart, yet a black colour will not be imparted to the skin. The blue colour is more or less intense, and permanent, according as the capillary vessels of the skin are more or less completely filled, but chiefly in proportion as the circulation is retarded through the lungs. The explanation of the other symptoms, the irregularities in the respiration, and in the arterial action, as well as the various affections of the brain, connected with over-distention or an imperfect supply of red blood, is sufficiently obvious."

With respect to the temperature of the body, Dr Farre considers an accurate comparative measurement of the *internal* and *external* heat, in these cases, as a desideratum; as well as a knowledge of the quantity of carbonic acid formed by respiration. From two cases, which he relates, it appears probable that the *internal* heat is higher, in these subjects, than in those who have perfect hearts, whilst the cutaneous temperature is contingent on external circumstances. Whence the author is led to the following reflections.

> "A still greater difficulty, however, presents itself with respect to the temperature of this boy; for the stream of blood, which fed the pulmonary artery, being in this case so very small, how could a sufficient quantity of caloric, in its specific form, be combined with it; to preserve a sensible temperature of at least one hundred degrees on the internal surfaces of his body. There seems to be a sufficient evidence that the function of respiration is a source of animal heat. But I am free to confess my opinion, that it is not its only source. Priestley's celebrated experiment proved, that black blood could be converted into red blood, without the actual contact of the air, or the structure of lungs. In like manner, although the lungs may be best adapted to perfect the changes which produce animal heat, it is sufficiently probable that this important operation does not exclusively belong to them.
>
> "The fact that this boy's internal temperature amounted to one hundred degrees, is in confirmation of the opinion that black differs in its capacity for heat from red blood; but if the former, on the internal surfaces, be capable of taking on a higher temperature, by two degrees, than the latter, it is also certain, that, on the external, it

is much more easily deprived of its heat, by the various circumstances which favour the reduction of animal temperature. So that, the latter, being able to retain its temperature longer than the former, must hold in combination an extra proportion of caloric.

"I have already attempted to reconcile the contradictory evidence respecting the symptom of coldness of the skin, by proving that its manifestation bears some proportion to the diminution of the pulmonary circulation. The last observation shews farther, that the coldness is regulated by external circumstances:—that, under those which favour the accumulation of animal heat, the thermometer at the skin rises as high, in such a subject, as in one of perfect structure; but that the heat is more slowly accumulated, and, under contrary circumstances, more rapidly expended. From this view we perceive, that a hasty observer, guided only by his sense of touch, on feeling the skin of a blue child, when *exposed*, would report it to be colder than natural; whilst another, making his observation on a similar subject, *well covered* and favourably situated for the accumulation of heat, would report the temperature of his skin to be as warm as natural." p. 41.

Of the second species, or, "malformations of the heart, or its arteries, *only* impeding the circulation of the blood," Dr Farre mentions but two varieties; and the first of these (II. *a.* "Left ostium ventriculi contracted, and mitral valve rigid,") is stated on the authority of the late Mr Allan Burns, till farther evidence shall decide, whether it be properly placed among the malformations, or whether it be not the result of disease; since chronic inflammation may produce such a change. The second variety, *b.* is that, in which the "ostium aortæ is narrowed by having two instead of three semilunar valves," and is very rare. Dr Baillie has described and figured it, and the author has here described and delineated another specimen.

In his concluding observations, Dr Farre admits that physiology is more likely to be advanced by the investigation of these malformations of the heart than pathology; but observes that the task of soothing and mitigating, as well as avoiding to inflict unnecessary pain, is a part of the physician's duty. There are two sources of distress from which these patients suffer; namely, in proportion to the diminished quantity of red blood which enters the general circulation, and to the mechanical impediment to the transmission of the blood, or to both conjoined. Referring the consideration of the latter to the subsequent essays, he proposes some inquiries as to the means of alleviating the former. The skin he believes to be in some measure a compensatory organ, by which the impeded function of respiration may be partially relieved; and from the palpable benefits derived from the warm bath, in the most severe paroxysms which the subjects of these malformations suffer, he deems it probable that their comfort may be promoted by living in a regulated atmosphere, and keeping the skin in that warm and moist state, in which it may in some degree compensate for the imperfect function of respiration. Something, he suggests, may also be done occasionally by modifying the position of the patient, to favour the influx of red blood into the aortic system. In this, however, the patient will commonly "minister to himself;" for the easiest posture in each case can only be learned from experience. The distress of these unhappy patients, may be likewise mitigated by regulating the kind and quantity of food, and the state of the bowels, by the mildest remedies.

It will be obvious, that our author has taken a very comprehensive, and at the same time a very detailed view of his subject, and has left nothing to be added, except what future investigation may bring forth. Considering the numerous illustrations by engravings, the price of this work appears to us below the ordinary average.

II.

Part First. An Essay on the Venereal Diseases, which have been confounded with Syphilis, and the Symptoms which exclusively arise from that Poison. Illustrated by Drawings of the Cutaneous Eruptions of true Syphilis, and the resembling Diseases. By RICHARD CARMICHAEL, M. R. I. A. President of the Royal College of Surgeons in Ireland, and one of the Surgeons of the Lock Hospital, Dublin. 4to. pp. 121. Longman and Co. London, 1814.

IN the midst of the perplexity and obscurity which prevail in the history of the diseases, which resemble syphilis, and under the difficulty which every practitioner must feel in establishing any satisfactory principle of diagnosis between them, an essay, emanating from such authority as the president of the Irish College of Surgeons, and a superintendent of the great Lock Hospital of Dublin, will necessarily attract the attention of every one who is zealous for professional improvement. We hastened to the perusal with considerable anticipations of deriving important instructions from such a teacher; for we acknowledge ourselves very much in need of instruction on this subject, and begin to despair of seeing the long-promised treatise on Lues venerea of Mr Pearson,* who is, we believe, better qualified than any other living practitioner to elucidate this obscure topic. We have been, however, rather interested by the novel views of the author, as far as they go, than instructed by his details, or convinced by his reasoning. For in order to adopt his limitations either of the true syphilitic or of the syphiloid affections, we must relinquish so much of what we apprehend we have learned from cautious observation, that our faith in medical evidence will nearly fail us. The author, indeed, labours under this material disadvantage, that his evidence is rendered defective, both by the absence of the plates, which should illustrate his positions, and by the non-appearance of the second part of his work, which may possibly detract from the force of his arguments.

It would appear, from the advertisement prefixed to this work, that this investigation was begun by *assuming* the principle, which is now maintained as the result of it. For, the author observes,

> "The manner in which the investigation was prosecuted, was on the most simple plan. Whenever a primary ulcer on the genitals occurred, which was destitute of the characteristic of chancre, *the hardened edge and base*, it was treated without the exhibition of mercury; and the same system was pursued in those cases of constitutional symptoms, which had a doubtful appearance. *The scaly syphilitic blotch*, as described in p. 129 of Willan on Cutaneous Diseases,† and the *excavated ulcer of the tonsil*, as described in p. 482 of Hunter, (Adams' edition) were alone esteemed to be syphilitic, and treated with mercury." p. 5.

Such a plan, if pursued without bias to a particular opinion, would be calculated to elicit truth; but that the investigation should terminate in confirming this limitation of true syphilitic eruption to a *scaly* blotch, to the exclusion of all other forms of cutaneous disease, appears to us extraordinary; and leads to the suspicion that the learned author was labouring to establish this, his preconceived opinion, and that, like other advocates of a favourite hypothesis, he has been a partial collector of the evidence. But we will not anticipate the question.

The author divides this first part of his essay into *five* chapters, the *first* of which contains "Observations on those morbid poisons, which stand in nearest relation to the syphilitic; and evidence of the existence of venereal diseases, which do not arise from that poison." The diseases first mentioned are the yaws, sivvens, and a Canadian malady, imperfectly described by Swediaur, in all of which the author observes a coincidence of character with the diseases often confounded with syphilis; especially the papular and pustular form of the eruptions, the accompanying pains of the joints, and the temporary relief which mercury occasions, but the recurrence and ultimate slow disappearance of the symptoms from the efforts of the constitution. He then proceeds to a detail of several authorities from Astruc, and of the substance of a paper of Mr Becket's, in the Philos. Transactions, in proof of the existence of several species of contagious ulceration of the genitals, before true syphilis was known; a circumstance well understood and admitted by every one acquainted with the history of this disease. Mr Hunter, with his peculiar sagacity in the observation of disease, was the first to demonstrate that such "venereal" ulcers, though not "syphilitic," (if we must use the paradoxical phraseology of the author) produce buboes and secondary symptoms, resembling those of syphilitic origin in our own times; and Mr Pearson observes, "many of the diseases of these parts described by ancient writers, do certainly still occur; and I believe myself warranted to suspect, that new forms of disease not unfrequently arise, which are succeeded by a regular series of symptoms, nearly resembling the progress of lues venerea."* Mr Abernethy has also illustrated the subject by an express treatise. These able observers, however, seem to have agreed in the inference, that, with respect to the secondary or constitutional symptoms at least, the resemblance is, in numerous instances, so close, that a diagnosis is only to be obtained by a combined view of the external character with that of the history of the rise and progress of the disease, and of the effects of remedies already tried;—an inference in which our own observation has led us entirely to concur. Mr Carmichael, however, maintains, that the secondary symptoms of syphilis are as definite as the primary chancre, being, in fact, limited to one form.

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* See his able and elegant work on Various Articles of the Materia Med. in the cure of Lues Venerea, *second* edition, *Introd.* p. 7.

† In the *second* edition of Willan it is there stated; but in the *first* it is described under the head of Psoriasis.

In Chap. II. he treats "of the local and constitutional symptoms of syphilis; the mode of treatment, and action of mercury on the system." After commending Mr Hunter's most accurate definition of chancre, as it occurs on the glans and prepuce, and pointing out the distinction of non-syphilitic ulcers on these parts; he observes, that chancres on the body of the penis are in some respects different, being of a dark livid colour, not excavated, and less indurated, and of a larger size, sometimes, indeed, extending round the body of the penis. Their edges are a little raised, and the surrounding induration very perceptible to the touch. He relates a case, to shew that if mercury is not employed, "the livid surface is alternated, every third or fourth day, with that of a light brown or tawny colour." The occurrence of this tawny hue he justly states as one of the diagnostics between chancre, so situated, and the phagedænic ulcer of the penis; and the other distinguishing circumstances are very clearly detailed. When phymosis and violent inflammation accompany chancre, and are attended with symptomatic fever, he urges the absolute necessity of general blood-letting, conjoined with the use of mercury, in order to prevent mortification; and he has illustrated the advantages of this depletory practice by several well-marked cases. After describing the results of such inflammation, especially the partial sloughing of the prepuce, and the formation of matter under the ligament, with the method of treatment, Mr Carmichael recommends a new application for venereal warts about the glans and prepuce not removable by scissars, which he affirms is much more efficacious than the ordinary remedies, savin, verdegris, and common caustics; namely, the *acetic acid.* It may be questioned, whether this has any advantages over the butter of antimony, which seldom fails to remove these excrescences.

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* See the treatise above quoted, Introduction, p. liii.

After an attempt to point out some circumstances for the purpose of establishing a diagnosis between syphilitic and other buboes, which, however, he admits to be next to impossible, the author proceeds to the examination of the secondary or constitutional symptoms of syphilis. The earliest intimation, he observes, that the system at large is affected, arises from "*the eruption* on the skin," and the ulceration of the throat, which are preceded by a slight febrile irritation, that continues, like a sort of hectic, as the symptoms proceed. Now, although the more we see of chronic diseases of the skin, accompanied by affections of the throat, and eyes, with pains in the joints and head, and a febrile erethism, the more we are disposed to limit the individual cases, that are really syphilitic, (i. e. for which mercury is the specific,) we cannot assent to this *conclusion* (with which our author seems to have *begun* his inquiries,) that "the true syphilitic eruption is" of *one form* only, and that "scaly." (p. 39.) Perhaps he will contemn our want of sagacity, when we admit that we have only attained to this perplexing inference, that the external character of these eruptions is not a sufficient criterion to found a diagnosis upon, without taking into consideration the history of their rise, and progress, and previous treatment. We were not a little surprised, indeed, on reaching that part of the treatise, which relates to the constitutional symptoms, originating from primary ulcers on the genitals which are not chancrous, and of which the author has described three species, to find, that these secondary symptoms are also limited to *one* species of eruption, and that of a *papular* character. (p. 81.) This eruption the author has represented in his engravings, to which we cannot refer: and the want of this assistance, as well as of the second part of his treatise, induces us to suppose that we may not thoroughly comprehend his views. Possibly he means to contend, that this papular eruption, and the secondary affection following the phagedænic ulcer of the genitals, (which is probably to be described in the second part,) are the sole eruptions, which are distinctly preceded by local ulcers upon the genitals; and that, in this work, he does not treat of those numerous forms of constitutional symptoms, to which Mr Pearson has alluded under the appellation of *cachexia syphiloidea*, many of which are not the consequence of local affections of the genitals, and which, therefore, could not be comprehended under the term *venereal*, in this new acceptation of the word.* If this be the author's view of the subject, he has no where given us any intimation of it. But even with this limitation, we cannot accede to his opinion. For what are we to say of those numerous cases, which we thoroughly believe we have seen, of pustular and tubercular eruptions, terminating in ulceration, which have distinctly followed primary affections of the genitals, and have yielded only to a mercurial course? We do not appeal to general experience, but to the few best informed practitioners; because we are certain, that multitudes of these eruptions are every day hastily pronounced to be syphilitic, which are not so; but to admit the statements of Mr Carmichael to the full extent, seems to be little less than giving up the evidence of our senses. The practice in these cases, indeed, would be greatly simplified, if the diagnosis were so clear as he represents it, and the varieties of disease so few as he describes; but there is an appearance of haste in the inferences, which he has deduced, as well as of a predisposition to deduce these particular inferences, which materially impairs the value of the evidence. The following is his account of the *venereal* eruption that is confounded with syphilis.

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* After stating that a distempered state of the body, adequate to the production of these spurious appearances, may be the offspring of other causes, or a consequence of diseases which have no affinity with Lues venerea, this excellent writer adds, "I have not yet attained to that complete and satisfactory knowledge of the *cachexia syphiloidea*, which would authorize me to obtrude a publication on the subject; but the experience I have already had in the treatment of that multiform disease, has taught me, that it may appear under the following different circumstances: 1. Where the syphilitic virus has lately existed in the constitution, and the patient has employed the accustomed course of mercury. 2. Where the patient has been repeatedly diseased with syphilis, and has used several courses of mercury. 3. Where a great length of time, from three to twelve, and sometimes twenty years, has elapsed since the patient has been exposed to the agency of the disease, and its remedy. 4. After a gonorrhæa, where small quantities of mercury have been used. 5. Where no venereal complaints, general nor local, have preceded the appearance of the cachexia syphiloidea; and where the patient has never been exposed to the hazard of contracting that disease, nor has laboured under complaints requiring the aid of mercury." Pearson, loc. cit. Introduction, page liv.

> "The constitutional symptoms, which arise from the superficial ulcer which is without elevated or indurated edges, and those which are derived from the excoriation of the glans and prepuce, attended with purulent discharge, and from gonorrhœa virulenta, I have already stated to be alike in all these complaints. They consist of more or less fever, which ushers in a *papular eruption*, inflammation, and soreness of the fauces, attended with difficulty of swallowing, severe pains which affect the head and larger joints, and sometimes inflammatory swellings over the superficial bones, which many would distinguish by the name of nodes." p. 81.
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> "In every instance, in which these complaints were attended with eruption, it was (except in two cases,) of the papular form, and of the genus Lichen; which, according to Willan, is, 'an extensive eruption of papulæ, connected with internal disorder, usually terminating in scurf, recurrent, not contagious.' The papulæ varied from a pale red to a deep crimson, as represented in Plate II., *Fig.* 5, 6, 7. Some of them were simply pimples, while others were almost advanced to the pustular form. The time of the appearance of this eruption after infection is probably as uncertain as the eruption of syphilis. In a few instances I have observed it to occur in four or five weeks after infection. The papulæ in some patients were numerous on every part, but particularly on the face, back, and belly. In others, they were more thinly scattered over the surface of the body. They do not all make their appearance together, as in the eruptions of the exanthemata, but follow each other in succession; so that on the same patient some spots will appear in their commencement like small pimples, others, which have arrived at maturity, form larger pimples, with acuminated tops, containing pus or lymph, while others, on their decline, consist of exfoliations of the cuticle. Their colour, in their latter stages, becomes paler, and assumes a copper tint, while the exfoliation of the cuticle gives an appearance of scaliness, a state in which it is most liable to be confounded with the scaly eruption of syphilis.

But they may be readily distinguished from each other; for when the papular eruption is on the decline, and has assumed a pale red or copper colour, on examining the patient, we shall find other spots in their papular or pustular form, which will at once point out the character of the eruption. But the very appearance of the declining papulæ will, to a discriminating eye, be sufficient for this purpose; for its copper-coloured scaly surface is more raised in the centre than its circumference, while the reverse is the case in the scaly leprous eruption of syphilis: as was more particularly insisted on when treating of that eruption."—pp. 62-3.

The author then refers to Plate II. fig. 5, 6, 7, where he has represented, he says, the most frequent appearance of the papular eruption, especially the colour and general dispersion of the spots, and their acuminated heads, terminating in small pustules. But why he should be in such haste to publish the letter-press, without these plates, and that letter-press containing only one part of his work, we know not. There is some nicety of discrimination exhibited in the above description, which is perspicuously worded; but we are not by any means satisfied that the diagnosis is so easily established even by a practised eye; or that, in fact, there are not true syphilitic *pustules*, or pustulating *papulæ* and small *tubercles*, of this character. We are persuaded that we have seen such eruptions, combined with other unequivocal symptoms of syphilis, (such as the inflammation of the iris, with an effusion of coagulable lymph upon it,) which have yielded presently to mercury; and we are sure that the ablest practitioners in this country, with opportunities even superior to those of our author, and probably much more protracted experience, and who at the same time are fully disposed to doubt the existence of true syphilis, and to ascribe all anomalous appearances to syphiloid cachexia, will coincide in our statement. We wish not to settle any pathological question by authority, which has been at all times the bane of intellectual improvement; but a question of fact, which this very nearly amounts to, (whether there are not both pustular and tubercular diseases, of clear syphilitic origin, and curable only by a mercurial course,) may be legitimately determined by such an appeal. But although we believe that our author has been premature in his decision, in thus limiting the secondary symptoms, both of syphilitic and syphiloid diseases, (for we like that distinction much better than that of syphilitic and venereal), we doubt not that his publication will be useful to the profession, a large majority of whom are constantly committing serious errors, by hastily pronouncing every eruption that is of a dark red hue, and accompanied with ulceration of the throat, pains of the head and limbs, and feverishness, to be syphilitic, and immediately pouring in mercury. For that we shall ultimately rest somewhere between these extremes, and very much nearer Mr Carmichael's position than to that of the majority, we are well convinced; and the nearer that experience shall lead us to the former, the more luminous will be our pathological views, and the more decided and beneficial our practice.

But to return to the constitutional symptoms of syphilis. The author proceeds to treat of the ulceration of the throat, and spontaneous ptyalism, and pains in the bones, periosteum, fasciæ, and ligaments. The spontaneous salivation he deems a secondary symptom of syphilis, and analagous to a similar occurrence in small-pox. But he probably never observed such a ptyalism in private practice, or in any other situation than in a syphilitic hospital or ward, where the very atmosphere, in the author's own language, is mercurial. The diagnosis which the author endeavours to establish between the true syphilitic node, and that which occurs in the resembling diseases, is correct, we believe, as far as it goes, but its practical application is not always clear. The slow progress, without discoloration, and with little pain in the early stage, which belong to syphilitic nodes, are not unfrequently observed in the syphiloid tumours; and such degrees of comparison are not easily ascertained. With respect to the syphilitic affections of the eye, the author refers to the treatise of the late Mr Saunders, for an anticipation of all that he could say on that subject.

Mr Carmichael treats cursorily of the complaints which originate from the use and abuse of mercury, especially of the mercurial phagedæna, pains in the joints, and nodes, which last, however, he is doubtful whether to ascribe to mercury. He contents himself with referring to the authors who have described the mercurial erethismus, and mercurial eczema; the former as mentioned by Mr Pearson, and the latter as described by Drs Alley, Moriarty, Spens, and Bateman, under the respective appellations of hydrargyria, lepra mercurialis, erythema mercuriale, and eczema rubrum. He might have observed, while quoting the 2d edition of Mr Pearson's work, that the disease is well described by him, under the name of eczema mercuriale. The author concludes this chapter with some judicious cautions respecting the necessity of a moderate introduction of mercury in the case of females, and of persons debilitated by previous courses of mercury, and other causes, with a view to avoid the excessive salivations, attended with dysentery, extreme debility, and sometimes dropsy, which are apt to ensue; and he relates some cases in illustration of his precepts.

In Chap. III. Mr Carmichael describes first the primary, and then the constitutional symptoms of one class of diseases which have been confounded with syphilis, leaving the second class to be discussed in the second part of his work. He maintains, that there are *six* primary symptoms, most of which are followed by constitutional symptoms, that have been confounded with those of syphilis. The first class, here treated of, composes *four* of these primary diseases, viz. "1. A superficial ulcer without induration, but with elevated edges. 2. A similar ulcer, destitute not only of induration, but of elevated edges. 3. An excoriation of the glans penis, and internal surface of the prepuce, attended with purulent discharge. 4. Gonorrhæa virulenta." The author adds, that he has not been able to trace any constitutional symptoms arising from the first species; but that those of the other three are precisely alike, and cannot in the slightest degree be distinguished from each other. They consist, in fact, he afterwards proceeds to shew, of the *papular* eruption, the description of which we have anticipated, and its concomitant symptoms. The second class, which is not here treated of, comprises the two remaining species, which the author calls, 1. the phagedænic ulcer, and 2. the sloughing ulcer. The constitutional symptoms, originating from the phagedænic ulcer, he affirms, are materially distinct from those which are produced by the first class of primary diseases; and he is disposed to believe, though he has but one case to ground his opinion upon, that the symptoms of the sloughing ulcer are the same.

Mr Carmichael infers, from his own experience, that at least five cases of these non-syphilitic ulcers occur, to one of true chancre. The *first* ulcer, abovementioned, which is level with the skin, or elevated above it, without induration or excavation, but with elevated edges, sometimes displaying a whitish, and sometimes a reddish brown surface, and beginning from a small itching pustule, is not followed by secondary symptoms. We apprehend this ulcer includes the ulceration produced by the Herpes præputialis, described by Dr Bateman in his Synopsis, the vesicles of which, if not seen within the two first days, might be deemed pustules. Some of the cases of this species of ulcer, however, are attended by obstinate buboes, and require sarsaparilla and antimony to be given internally. Externally our author relied on lotions of the oxymuriate or muriate of mercury in lime-water, a grain of the former, or ten of the latter, being added to the ounce; or on the compound of spirit of lavender, alone, or diluted. Caustics, he affirms, produce no beneficial effects; do they produce no harm? The author has noted 26 cases of this ulcer, thus treated successfully without mercury, and in which, as far as he knows, no constitutional symptoms followed. Five of them are here detailed.

The second species of non-syphilitic ulcer, which is destitute both of induration and of elevated edges, is thus described.

> "In general, it is raised above the suronnding skin, and exhibits a smooth surface, the colour of a healthy sore, but without granulations, and has somewhat a fungous appearance. Sometimes it is on a level with the surrounding surface, and seldom is excavated. These ulcers, which are far more general than any other, to which the parts of generation are liable, vary from the size of a pea to that of half-a-crown, and are more frequently found on the glans and internal surface of the prepuce than elsewhere, in which situation they in general excite phymosis; and it is, therefore, extremely probable, that this is the identical ulcer mentioned by Celsus, as inducing phymosis, and readily cured by lenient remedies, and mild detergent washes. They also occur on the external surface of the prepuce, body of the penis, and scrotum, in which last situation they are considerably raised above the surrounding surface, so as to resemble fungi or soft warts, and are rapidly cured by an application of a strong solution of muriate of mercury, in the proportion of two or three grains to an ounce of water. In women they usually occur on the labia, perineum, and fossa of the nates." p. 67.

The author remarks, that there is great reason to suppose that this ulcer, and the third species or excoriation, both of which often accompany gonorrhæa, are in fact the result of the same poison with gonorrhæa; and that, when phymosis has occurred, it is often impossible to determine from which of these three sources the discharge proceeds; in fact, that it often arises from all the three sources at the same time. He relates four cases, to illustrate the speedy care of these superficial ulcers by means of the mercurial washes just mentioned, with antimonials and sarsaparilla internally.

The *third* species of non-syphilitic ulcer, which has by some once been called chancrous excoriation, and by others spurious gonorrhæa, is an "excoriation of the glans penis, and internal surface of the prepuce, attended with purulent discharge," p. 71. This affection, the author adds, usually occasions phymosis, and in more than two thirds of the cases which he has observed, was accompanied by gonorrhæa. "When the prepuce can be retracted," he observes, "and the matter washed off, the surface of the glans and prepuce does not appear, on close examination, to be wholly excoriated, but the excoriation occurs in patches, with intervening spots of sound cuticle. The corona glandis was, in every instance, the part most severely affected, and the latest to recover from the disease. It yields to the same plan of treatment as was employed in the preceding species of ulcer."

The *fourth* primary symptom, which is sometimes followed by constitutional affections that are mistaken for syphilis, is *gonorrhæa*. The author observes, that Dr Monro and Mr Whately have maintained, that even the urethra is not inflamed universally, but in patches, like the glans and prepuce, as above described; which is another argument for the identity of the poison of gonorrhæa, and of the excoriation just mentioned. The author has entered fully and clearly into the refutation of the identity of the poison of gonorrhæa and syphilis,—a refutation which we presume all thinking practitioners of the present day will deem nearly superfluous. In the concluding argument Mr Carmichael maintains, that he has seen a great many cases of gonorrhæa alone in females (males affected with gonorrhæa alone are not admitted into Lock Hospital) in which the same papular eruption, which we have described above, concurred; that this concurrence of the eruption is common with gonorrhæa and the excoriation of the glans and prepuce, and also with the superficial ulcer; and consequently that one and the same "venereal" poison is the source of these three forms of primary disease, all of which are followed by the same constitutional symptoms: whereas the "syphilitic" poison is altogether distinct, both in the primary and constitutional effects which it excites.

With respect to the treatment of gonorrhæa, although the author admits the general inefficacy of astringent injections, containing the acetates of lead and zinc, the sulphate of zinc, &c.; and even allows that the imprudent use of them may occasion chordee, inflammation of the neck of the bladder, or of the testes, and strictures in the urethra; yet he believes that strictures at least are as frequently seen where no injections had been used; and he speaks with great confidence of the efficacy and impunity, with which injections of the oxymuriate of mercury in lime-water are employed. He begins with the proportion of one grain of the corrosive muriate to six ounces of the liquid, and increases to three grains in the same quantity. Now we really think it necessary to caution the inexperienced reader against at least the larger proportion; for even the smaller has most commonly produced violent excitement in our hands, and the larger we are persuaded will produce a dangerous inflammation, especially if thrown into the male urethra. We have seen the solution, recommended by Mr Whately, extremely beneficial; but this stronger solution has always seemed injurious.

This part is concluded by an account of the secondary symptoms of these gonorrhæal ulcerations, which we have already anticipated, and by a long detail of cases, illustrative of the varieties which the constitutional diseases assume, and of the successful method of treatment. The cases are well detailed, and cannot be perused without interest. The *fourth* and *fifth* chapters remain to be completed in the II. part.

Mr Carmichael's views, then, to which we must give the merit of considerable originality, may be thus recapitulated. He contends that there are at least *three* poisons which produce primary local symptoms, followed by secondary constitutional disease. 1. The *syphilitic*, characterized by chancre, followed by a *scaly* eruption; 2. the *gonorrhæal*, characterized either by a superficial ulcer, destitute of induration and of elevated edges, or by a patchy excoriation of the glans and prepuce, or by a proper gonorrhœa from the urethra, each of which is liable to be followed by a *papular* erruption, with similar pains, &c. as in the syphilitic; and 3. another poison, the effects of which remain to be described, which is characterized by the *phagedænic* and perhaps also by the *sloughing* primary ulcer, and is followed by a particular set of secondary symptoms. There is besides a superficial ulcer, with elevated edges, which is not followed by secondary symptoms, and which it would have been perhaps better not to have classed with the gonorrhæal. This third poison will be the subject of the *fourth* chapter. We find, too, on recurring to the table of contents, that the author intends to treat in his *fifth* chapter of "constitutional symptoms, which have not been traced to their primary ulcers," which, we suppose, will include an inquiry into the cachexia syphiloidea.

If the author should demonstrably make out his discrimination of these secondary syphiloïd symptoms to this extent, we will retract all the doubts and objections that we have advanced in the early part of this review, and hail our magnus Apollo. Many of his observations, especially in regard to the diagnosis of appearances, impress us with the character of great acuteness, which detects differences which it is not easy to communicate; and every part of the work evinces that the author thinks for himself. At the same time, however, our confidence in his statements, we must acknowledge, is something diminished, by certain symptoms of careless generalization from a few facts, and certain differences thence arising with respect to facts apparently well established; such, for one instance, as to the quantity of corrosive sublimate bearable in a common injection; to say nothing of the apparent assumption of the hypothesis from the beginning. These are the foundation of much distrust. Nevertheless, we wait with eagerness the appearance of Mr Carmichael's plates, and of the second part of this treatise; and in the meantime we consider the profession as indebted to him, for the new views of this complicated subject which he has presented to us; and doubt not that the path of investigation, which he has opened, will lead to valuable knowledge, if it should not conduct us exactly to the objects which he has endeavoured to descry.

III.

Traité des Poisons, tirés des Règnes Minéral, Végétal, et Animal, ou Toxicologie Générale, considérée sous les Rapports de la Physiologie, de la Pathologie, et de la Médecine légale. Par M. P. Orfila, Naturaliste Pensionnaire d'Espagne, Docteur en Médecine de la Faculté de Paris, Professeur de Chimie et de Physique; Précédé du Rapport fait à la Classe des Sciences Physiques et Mathématiques de l'Institut de France.—Tome I. Ire Partie. pp. 304. IIe. Partie. pp. 325. 8vo. Paris, 1814. Crochard.

These two volumes treat of the corrosive and exsiccant poisons, and are but part of a complete treatise on poisons which the author intends to publish. From the specimen he has given us, we shall be impatient to possess the whole, although we can by no means agree with the committee of the Institute of France, appointed to report on M. Orfila's manuscript, in the general censure thrown upon all preceding writers on toxicology. It is evident that both the author and the reporters were unacquainted with some of the best writers on the subject, and especially with the laborious and correct compilations of Gmelin,[*] whose research scarcely a single fact then known has escaped. In correcting this historical error, we have not the least intention of detracting from M. Orfila's merit, which we fully appreciate.

In his general arrangement and classification of poisons, M.

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[*] Gmelin (Johann Friedrich) Allgemeine Geschichte der Pflanzengifte. Zweite vermehrte Auflage. 852 pp. 8vo, Nurnberg, 1803.

——————Allgemeine Geschichte der thierischen und mineralischen Gifte; mit einer Vorrede von Blumenbach. Zweite Auflage. 391 pp. 8vo, Erfurt, 1811.

Orfila has followed Fodéré; but as he seems to be sensible of its errors, and only to have adopted it because he was not prepared to substitute one of his own, we shall not stop to criticise it.

M. Orfila begins his work on corrosive poisons, with an account of their *general* action, the general symptoms, and the organic lesions produced by them, and their general treatment. In this part of his work M. Orfila has completely failed, because we believe it is impossible to succeed in it. The action of each poison is so exceedingly various, according to the circumstances under which it is exhibited, and the state of the system on which it acts, that it seldom admits of being generalized; but a general description of the effects and treatment of corrosive poisons, under which denomination are included many substances, having very little analogy in their action, and some acting upon totally different principles, must either be so vague as to convey no information, or must consist of an assemblage of errors.

In treating of the individual poisons, M. Orfila speaks in succession of their chemical and physical properties—their physiological action—the general symptoms they produce, illustrated by cases—the organic lesions they produce—the application of the preceding facts to the different cases which may occur in medico-juridical practice, and the manner of counteracting their effects, with an inquiry into their alleged antidotes.

The facts relating to the physical and chemical history of each poison are well chosen, and perspicuously narrated and explained; but we were particularly pleased with finding an accurate account of their habitudes, with many vegetable and animal matters, for which the best systematic works on chemistry will be consulted in vain, although their knowledge is of the greatest importance to the juridical physician. Every chemical writer describes the manner of detecting the usual mineral poisons, and their action upon various reagents; but their description is taken from experiments made upon the substance in a simple state, or dissolved in water. In juridical practice the circumstances are totally different. The substance and its quantity are not only unknown, but it is almost always mixed with other substances, which suspend or alter the common effects of reagents upon it; so that our knowledge of pure chemistry may sometimes mislead us. The perusal of M. Orfila's work cannot fail of convincing every physician of the necessity of studying the objects of juridical chemistry more minutely than the general chemist will ever condescend to do.

Corrosive sublimate is decomposed, sooner or later, and converted into the submuriate, by almost vegetable substances, distilled waters, extracts, oils, syrups, honeys, gums, and alcohol.

Its action upon albumen is so marked, that it has been proposed by Dr Bostock as the most delicate test of that animal oxide; but in sufficient quantity it also coagulates milk, and causes a precipitate with gelatine, and in beef-tea. It is precipitated red by osmazome, and orange by bile; and it acts remarkably on fibrine or flesh, corrugating and hardening it. But the manner in which the presence of these substances alters the action of the reagents or tests is still more worthy of notice. The precipitate obtained from mixtures of corrosive sublimate, with boiled beans and broth, or albumen, vegetable broth, boiled apples, tea and sugar, was not blackened by lying in lime-water for two days, (p. 83, Vol. I.) yet yielded mercury by sublimation. Dissolved in Burgundy wine, it was precipitated black by potass, very dark green by ammonia, blue, passing to violet by prussiate of potass, and syrup of violets was changed by it to green. Dissolved in beef-tea, it is precipitated white, or grey or black by potass, and dirty white or yellowish by lime-water, and the addition of bile to a solution of corrosive sublimate prevented it from being affected by ammonia or potass.

In regard to arsenic, the necessity of being able to detect it, when disguised by the presence of other matters, is more obvious, as it does not discover itself by its taste, and is therefore more likely to be employed for committing murder than any other mineral poison. Solutions of albumen, or gelatine, and common beef-tea, and bile, were not affected by the addition of arsenic, and did not affect considerably the action of the common reagents. Strong tea, however, completely altered them; so that nitrate of silver gave a yellowish white precipitate, which became instantly black, lime-water a dirty canary yellow, and sulphate of copper a reddish violet; sulphuretted hydrogen alone furnished its usual yellow precipitate. Decoction of coffee with arsenic was precipitated of a deep yellow by nitrate of silver, of a grass-green by ammonia-sulphate of copper, of a golden yellow by sulphuretted hydrogen, and yellow by lime-water. Wine with arsenic gave deep yellow by sulphuretted hydrogen, bluish-black by ammonia-sulphate of copper, and white by nitrate of silver: with a larger proportion of arsenic, the copper yielded a green precipitate. Arsenic mixed with milk was also difficult to detect. All these mixtures, evaporated to dryness, and treated with distilled water, indicated the presence of arsenic, by one or other of these tests; and from all of them, metallic arsenic was obtained, by reduction with potass and charcoal. These are all the facts of this nature in regard to arsenic given by M. Orfila, but they are far from being sufficient; and we regret that want of room obliges us to postpone many corrections and additions which our experience could afford.

Tartar emetic gives an orange precipitate, containing oxide of antimony, when mixed with vegetable juices and decoctions. The tincture of gall-nuts is its most delicate test, and furnishes a copious curdy dirty yellow precipitate. One part of a saturated solution of tartar emetic, mixed with ten of red wine, remains transparent, but the mixture yields a reddish yellow precipitate, on the addition of some drops of hydrosulphuret of ammonia, or hydrosulphuretted water, and becomes green on the addition of a larger quantity of the reagent; with tincture of galls the precipitate is clear violet, and with sulphuric acid deep violet. One part of the solution, with ten of strong tea, remains transparent, but the mixture is precipitated deep red by hydrosulphuret of ammonia, and yellowish-white by sulphuric acid; but is merely rendered turbid by tincture of galls. Albuminous or gelatinous fluids or bile do not seem to alter the effects of the reagents in this case.

A solution of nitrate of copper is precipitated of a reddish yellow by tea. When mixed with red wine, in the proportion of one to ten, it remains transparent, but the mixture is precipitated black by the hydrosulphurets, marron brown by the precipitate of potass, and very dark grey by ammonia, which does not redissolve the precipitate entirely, and never gives a blue colour to the fluid. Acetate of copper forms a coagulum in albuminous fluids and milk, and it seems to be rendered insoluble in water by being triturated with sugar.

Muriate of tin decomposes almost all animal and vegetable fluids, except simple syrup.

Sulphate of zinc in solution is not affected by syrup or Burgundy wine, but is precipitated deep blue by tea and galls, white by albumen and gelatine, yellow by bile, and curdles milk.

Nitrate of silver in solution is not affected by tincture of gall-nuts or gelatine; but it changes the colour of tea to red and black, and precipitates albumen, gelatine, milk, and bile. Mixed with Burgundy, it is precipitated of a violet colour instead of yellow, by phosphat of soda.

Muriate of gold in solution is not affected by syrup; is precipitated reddish yellow by tea; red, chocolate, or metallic, by tincture of galls; deep purple and metallic, by Burgundy; yellowish, by albumen and gelatine; green, passing to purple and violet, by bile; and curdles milk.

Nitrate of bismuth is not affected by gelatine; is precipitated yellowish white by tincture of galls and strong tea; yellow, by bile, and coagulates albumen and milk. Burgundy gives a precipitate of a rosy or violet colour; and the supernatant liquor, which is not discoloured, loses its colour on the addition of ammonia, without yielding a white precipitate, although the hydrosulphurets prove the presence of bismuth.

Acetate of lead is not affected by gelatine; is precipitated by bile and wine, and yellowish white, by tincture of galls, or tea; coagulates albumen and milk. Burgundy, containing lead, loses colour, and gives a dirty yellow, instead of a white precipitate with ammonia; and although it is precipitated by the hydrosulphurets, the wine unimpregnated by the lead is affected in the same way; but hydrosulphuretted water precipitates the poisoned, and does not affect the pure wine.

As connected with the chemical history of poisons, we may here notice M. Orfila's directions for detecting them, chiefly for the sake of noticing the methodical manner in which he treats the subject. In regard to each principal poison, he describes minutely the line of conduct to be followed in four different cases; when the individual is still alive, and the remains of the poison can be procured; when he is alive, and there are no remains, but the matters vomited or passed by stool can be had; when he is alive, and there are neither remains nor egesta; and, lastly, when he is dead. M. Orfila duly appreciates the value of the processes recommended by different chemists, and describes minutely the precautions to be taken, in order to render the examination in every possible case satisfactory. It would not be proper at present to enter into any detail upon this extensive subject; but we cannot omit noticing that M. Orfila was able, in several instances, to detect poison, both in the egesta, and when combined with the substance of the stomach.

Our readers will perceive the value of these facts, although we have been obliged to state them in the most concise manner, and to omit many altogether, that we may leave room for some account of a series of still more interesting observations on the actions of all the various corrosive poisons upon the animal economy; a subject upon which there are still many popular errors, and great obscurity. Upon each principal poison, M. Orfila arranges his observations under three heads,—its action upon the animal economy, when taken into the stomach, injected into the veins, or applied externally;—the general symptoms produced by it, illustrated by cases;—and the organic lesions to which it gives rise.

In order to ascertain the manner in which each poison produces death, M. Orfila has been obliged to perform a vast number of original experiments, which constitute a real addition to our knowledge of the subject. We conceive ourselves entitled to speak with confidence in regard to our author's merits, because we have been and are engaged in researches of a similar nature, and know their difficulty.

M. Orfila made experiments in several ways. He introduced the poison into the stomach in various doses, without in any way injuring the animal. Where the event was fatal, this mode of experimenting is without objection, and satisfactory; but when the substance is quickly rejected by vomiting, we are not entitled to pronounce it innocent, because no other bad effects follow. It is from not attending to this circumstance that many alleged antidotes have acquired unmerited reputation. In order to obviate this objection, M. Orfila, in many cases, injected the poison through a hole made in the œsophagus, and then put a ligature around that canal, to prevent vomiting. In some cases, no doubt, he was in this way enabled to attain his object; but the method is liable to very great objections, as the animal is subjected to considerable injury in detaching and tying the œsophagus, and will suffer much distress from being prevented from vomiting, even supposing the substance not to be poisonous; and accordingly we find M. Orfila himself admitting that some animals, on whom the operation was merely performed without any poison being administered, died sooner than others who get poison at the same time. From our own experiments, we are satisfied, that, by much the best method of ascertaining the poisonous nature of any substance, and of tracing its effects upon the system, is, to apply it to a slight wound in some unimportant part, reaching to the subcutaneous cellular membrane. We are thus able to distinguish accurately between its local and general effects; and it is astonishing how small a quantity of a really poisonous substance, applied in this way, will produce death. This method has not been altogether neglected by M. Orfila, but only not carried to the extent it deserves. Another mode of experimenting used by M. Orfila, was, the direct injection of the suspected substance into the circulating system, by throwing it into the jugular vein. The results are undoubtedly interesting, but they are not conclusive as to the poisonous nature of any substance, for they may kill in other ways; for example, by chemical action coagulating the blood,—and yet be perfectly innocent, taken into the stomach, or inserted into a wound. We may also here notice an error into which M. Orfila has sometimes fallen, in concluding that, because a substance, injected into a vein, produces different and much more violent symptoms than when taken into the stomach, it does act by absorption. But surely there is a very wide difference between the action of a substance rudely thrown into the circulating mass, and one gradually taken and insensibly blended with it. Besides, the substance may not be absorbed entire, and unchanged; and yet we are warranted, in common language, to say, that it acts by absorption, if the essential constituent by which it acts on the constitution be absorbed.

We therefore consider Dr Lafort's proof against the absorption of corrosive sublimate, as being totally unfounded; nor do we acquiesce in Mr Brodie's, that, when it destroys the inner membrane of the stomach, the idea of absorption is precluded. That it may, and occasionally does kill, by disorganizing the stomach, and the violent impression it makes upon it, independently of absorption, just as a child is sometimes killed by a large draught of boiling water, we admit; but there are abundance of facts, which prove that it may be absorbed even when the part to which it is applied is disorganized. M. Orfila also seems to adopt Mr Brodie's opinion, in regard to the immediate cause of death from corrosive sublimate; that it was not in the stomach, because its functions may be suspended for days, without death being produced, but that it was in both the brain and the heart. We doubt much, however, whether we are yet so far advanced in physiological science, as to adopt his conclusions, or admit the validity of his arguments. But this subject would require a very long discussion. We therefore postpone it to another opportunity, and return to M. Orfila, and the immediate effects of the various poisons he has examined.

The modes of action of corrosive sublimate, may be summed up in the following theses:

1. Corrosive sublimate acts instantaneously, as a chemical agent, on the part to which it is applied, corroding or disorganizing it. This action may be so intense, as to produce almost instantaneous death, (Sproegel, Experiment 4.) Where the animal survives the first impression, its death is the consequence of the inflammation produced, and is seldom very speedy.

2. Corrosive sublimate, and, indeed, every mercurial, is apt to produce a specific excitement of the system, of which salivation is a peculiar symptom; and this action may also be so excessive as to produce death.

The action of arsenic is much more obscure; but we know that it acts in a totally different manner, and upon different principles from corrosive sublimate. It is *not* a chemical caustic, and ought to be removed from among the corrosive or escharotic poisons, with which it has hitherto been classed by all systematic writers, even M. Orfila, in whose description of the organic lesions produced by it, the bad effects of this error are evident. The primary action of arsenious acid, we think, may be summed up in the following theses, chiefly the result of a great variety of experiments, made by the late ingenious Dr Campbell, at most of which we assisted:—

1. It does not act chemically on animal matter, living or dead.

2. Its chief effect is to produce a disease somewhat analogous to cholera morbus, whether it be taken directly into the stomach itself, or inserted into the subcutaneous cellular membrane of a remote part, or applied to a delicate membrane. In some few cases, where the action of the poison is most intense, death occurs from the sickness or fainting, without vomiting or purging.

3. Frequently a considerable interval intervenes between its being received, even in solution, into the stomach, and its action.

4. Neither paralysis of the voluntary muscles, nor convulsions, nor delirium, nor coma, nor disordered respiration or circulation, are ordinary symptoms of the disease produced by arsenic.

5. After death, we were frequently unable to discover any organic lesion, and we generally found that the inflammation was less, in proportion as the arsenic was more speedily fatal.

Copper. M. Orfila has repeatedly had opportunities of giving verdigris and acetate of copper to dogs of different size, and he has constantly remarked, that when the dose of the latter exceeded twelve or fifteen grains, that the animals perished in less than three quarters of an hour. The symptoms which preceded death, were abundant vomitings of a bluish matter, evidently coloured by the metallic salt; fruitless retchings, after he had emptied his stomach of its contents; plaintive cries; great difficulty of respiration; irregularity and quickness of pulse; frequently general insensibility; the animal couched as if dead; almost always he was convulsed; and some moments before death, general rigidity, tetanic struggles, and a great deal of foam at the mouth, were observed. On examining the body immediately after death, no contractility remained in the muscles; the mucous membrane of the stomach was covered with a bluish layer, containing a portion of the ingesta, (*matière ingerée*), which layer was hard, as if horny, and when scraped, the mucous membrane below was of a rose colour. The trachea and bronchi were filled with white froth; the lungs crepitated, and exhibited some red points, upon a pale ground. The heart had ceased to beat.

The injection into the jugular, of a grain of acetate of copper in half an ounce of water, commonly causes death in ten or twelve minutes. The animal at first performs the motions of mastication and deglutition, and afterwards vomits with violent efforts; he respires with great difficulty, and is very violently convulsed; he lies down,—suddenly becomes insensible,—the rattle comes on, and he dies. On opening him, nothing remarkable is perceived in the *primæ viæ;* the irritability of the muscles seems extinguished; there is no change in the lungs, and the heart is without action.

Tin. Three-fourths of a grain of muriate of tin, in solution, injected in small quantity into the jugular vein, produced paralysis and insensibility; two grains tetanus; and six grains speedy death after vertigo. On dissection, the lungs were more or less shrivelled, and partially gorged with blood; the blood itself was dark coloured, and there was a slight redness of the mucous membrane of the stomach and duodenum. Muriate of tin, introduced into the stomach, excited violent vomiting, great depression, and death without paralysis or convulsions. On dissection, the stomach was indurated, and as if tanned; ulcerated in various parts, and of a dark red colour; and the intestinal canal contained much black thick ropy bile. The lungs were sound.

Silver. A very small quantity of nitrate of silver in solution, injected into the jugular, produced difficulty of breathing, threatening suffocation, retching, pain, convulsions, and death. On dissection, the lungs were found gorged with blood, and the heart distended with black blood. A dog took into the stomach twenty grains, dissolved in seven drachms of water, which only produced uneasiness and depression; in two days, it got thirty-two grains more, which excited frequent vomiting, from which it speedily recovered. On the fifth day, 36 grains were thrown into the stomach, through a hole in the œsophagus, which was immediately tied to prevent vomiting; the animal instantly suffered excruciating pain, which ceased in three hours, and died in the night of the following day. The whole mucous membrane of the stomach was dissolved into a pulp, but the lungs were healthy.

Zinc. A contracted solution of zinc, injected into the jugular vein, produces retching, coma, and death. On dissection, no morbid alteration was discovered. Taken in large quantities into the stomach, it only acted as an emetic, accompanied with temporary uneasiness and depression; but when the vomiting was prevented, by putting a ligature on the œsophagus, the animal died, and the stomach was everywhere of a dark red colour; and the lungs were a little darker than usual.

Gold. The muriate in solution, injected into the jugular, killed, apparently by inducing suffocation; it also excited vomiting. The lungs were gorged with blood, so as to sink in water, and the heart was full of black blood. Taken into the stomach, muriate of gold proves fatal in consequence of the inflammation it excites.

Bismuth. The supernitrate injected into the jugular vein, produced retching, plaintive cries, convulsions of the limbs, palpitation, difficulty of breathing, and general depression; or vertigo, and intoxication, according to the quantity. The lungs were dark coloured, but turgid only in small portions, or wrinkled, and the left ventricle and arteries contained only a little black blood. The subnitrate of bismuth, introduced into the stomach, produced nausea, and vomiting of white ropy matter, difficult, noisy, and deep respiration, trembling of the limbs, and death. The mucous membrane of the stomach was universally inflamed or destroyed, and portions of the lungs gorged with blood, so as to resemble liver.

Acids. The sulphuric, nitric, muriatic, and phosphoric, acids, when injected into the jugular vein, caused death by coagulating the blood; when introduced into the stomach, they killed by the inflammation and disorganization of that organ; and when applied to the skin, by the burn which they produced, or the suppuration which was the consequence of it. The nitrous, fluoric, sulphurous, phosphorous, oxalic, and tartaric acids, acted in the same manner.

Alkalis. Potass, soda, and ammonia, and their subcarbonates, act also in the same way. They coagulate the blood when injected into the jugular, and they corrode the stomach; but ammonia also produces tetanus and violent convulsions.

Barytes, pure or carbonated, introduced into the stomach, caused vomiting, hiccup, insensibility, convulsions, and death, The stomach was inflamed and corroded, the lungs nearly natural. The muriate of barytes, injected into the jugular, caused great agitation, and convulsions. The respiration was not affected. The heart was distended by coagulated blood, the stomach was healthy, and the lungs nearly natural; introduced into the stomach, it produced vomiting and purging, violent convulsions of the anterior extremities and face, and greatly accelerated pulse. The stomach was of a livid red, and the mucous coat could be easily rubbed off. The left ventricle of the heart contained black fluid blood.

Lime was introduced into the stomach of a little dog, to the extent of a drachm and a half in powder. It excited vomiting, and the discharge of much saliva, but caused little distress, and the dog soon seemed to recover. In five days, it got three drachms, which caused it to vomit quickly, and to be depressed, and it died in three days, without having experienced convulsions, or vertigo, or paralysis. The pharynx and œsophagus were slightly inflamed; and the mucous membrane of the stomach was inflamed throughout its whole extent.

Phosphorus, dissolved in oil, injected into the jugular vein, instantly produced exhalations of phosphorous acid by the mouth and nostrils, caused difficult respiration, and panting, and death, after having rejected a great quantity of bloody serosity, without any remarkable nervous symptom. The left ventricle of the heart contained blood as black and fluid as that which filled the right. There were several livid and dense portions of the lungs; the stomach was natural. Phosphorus introduced in small lumps into the stomach, does not at first induce any remarkable effect, but the animal falls gradually into a state of depression, and dies. The stomach is much inflamed, and contains a ropy flocculent fluid. When it is introduced into the stomach, dissolved in oil, its action is much more violent. Fumes of phosphorus and gas were exhaled from the lungs; the animal seemed to suffer exquisite torture. It then lay immoveable, and was violently convulsed before death. The stomach was perfectly disorganised and corroded. The lungs were red, gorged with blood, and did not crepitate.

Powdered glass, taken into the stomach, produced no effect.

Cantharides in tincture, injected into the jugular vein, produced vertigo, stupor, and death. The blood in the left ventricle of the heart was fluid and reddish; that in the right, black, containing coagula; but exactly the same effects were produced by the injection of alcohol alone. Oil digested upon cantharides, was next tried. It soon deprived the animal of sensibility and muscular power, and induced successively tetanus, convulsions, difficult respiration, and death. The lungs were very large, and gorged with a great quantity of reddish serosity; in some parts they were livid and compact. The mucous membrane of the bladder was slightly red, and the stomach natural. Cantharides in powder, taken into the stomach, acted as a corrosive, producing vomiting, the discharge of much bloody mucus, depression of vital power, and death. The stomach is always violently inflamed, and the bladder sometimes.

Lead. The solution of the acetate, to the extent of three grains, injected into the jugular, caused only slight vomiting of white ropy matter. Five grains seemed, at first, to have no effect, but in three days it became depressed, and refused its food, but preserved the power of walking. On the fourth day, its gait was unsteady and difficult; its hind legs were slightly convulsed; it was very weak, and it died on the fifth day. The lungs and stomach were healthy. Thirteen grains killed instantly, without any mark of pain or convulsion, after three or four deep inspirations. The blood in the left ventricle was fluid, and of a florid red. When taken into the stomach, in powder, and in considerable quantity, it causes death, after vomiting, insensibility. The stomach was inflamed. Given in solution, it also excited nervous symptoms, loss of muscular power, convulsive trembling of the limbs, and vertigo.

Hydrosulphuret of potass. In solution injected into the jugular it produced immediate tetanus, from which, in one instance, the animal quickly recovered, and in another he perished. The blood in the heart was florid, and in the left ventricle of a deep red. Taken into the stomach it excites vomiting, difficult respiration and depression, or, when vomiting was prevented, violent attempts to vomit, hurried respiration, panting, loss of muscular power, convulsions, tetanus, death. The stomach was found much inflamed and ulcerated, and the lungs partially gorged. The left heart contained black blood.

Iodine. M. Orfila is assuredly the first to have tried the effects of this singular and costly substance upon the animal economy, both on dogs and on himself. He first took two grains fasting, but they only excited an abominable taste, and some nausea. Next day he took four grains. He was immediately sensible of constriction and heat in the throat, which lasted a quarter of an hour, and he soon vomited yellow liquid matter, in which iodine was easily recognized. Two days after he took six grains, which instantly excited heat and constriction of the throat, nausea, irritation, salivation, and pain of stomach; and in ten minutes copious bilious vomiting, and slight colic pains, which yielded to two emollient clysters, after having lasted an hour; the pulse rose from 70 to 90, and was fuller. Next day he only felt slightly fatigued. Dogs could bear about a drachm and a half of it, if they were permitted to vomit freely; but if the œsophagus was tied, it proved invariably fatal, after exciting violent efforts to vomit, hiccup, thirst, quick pulse, and great depression. The edges of the rugæ of the stomach were always corroded and ulcerated. The lungs and other organs natural. A large quantity, two or three drachms, proved fatal to dogs in the same way, although the œsophagus was not tied; and it is worth remarking, that M. Orfila was able to detect iodine in the bluish pasty matter accompanying the stools which were first passed after taking it, or in the first matters vomited, which were always tinged yellow. One drachm twelve grains of iodine applied to a wound in the back of a dog, acted merely as a caustic, causing a thick yellowish white eschar to slough off in three days.

To these inquiries into the mode of action of each poison upon the animal economy, M. Orfila subjoins some cases, original and selected, in which their deleterious action upon the human body was evinced, and, from the whole, he draws up a summary of the organic lesions produced by each.

M. Orfila's work also contains another series of interesting and original experiments, on the antidotes of the several poisons mentioned in it, as forming a branch of their treatment. On the properties which a substance ought to possess, in order to consitute it an antidote, he agrees with Renault*, except in not requiring it to be soluble in water and animal fluids, since magnesia, the best antidote of the mineral poisons, is not soluble, and he might have also struck out from among the requisites, their being perfectly innocent when taken in large doses; for both the alkalis and mineral acids, although poisonous when given in large doses, may be legitimately considered as antidotes to each other, and are in fact used as such. In experimenting upon poisons and their antidotes, no positive conclusion can be drawn, unless the œsophagus be tied, as the recovery of the animal may be owing to the rejection of the poison by vomiting, while, on the other hand, we must be equally cautious in drawing positive conclusions from the comparative time animals survive the operation; for animals differ so much in vitality, that one to whom no poison has been given, shall die of the effects of tying the œsophagus, sooner than another to which also a dose of poison has been administered. Hence M. Orfila concludes, that "When it is established that any poison produces inflammation, ulceration, or erosion of any part of the alimentary canal, we may, without hesitation, pronounce any chemical reagent, which prevents the occurrence of these effects, to be its antidote, at whatever period, death may succeed the experiment." (Vol. II. p. 48.) Proceeding upon these principles, he rejects many of the antidotes proposed by Navier and others, and has established the virtues of others. The great length of this article will only permit us to attend to the latter, the knowledge of which cannot be too generally diffused. Albumen he finds, by experiment, to be the only counter-poison of corrosive sublimate, and, in fact, taken in sufficient quantity, it decomposes the metallic salt, forming a triple compound, consisting of albumen, muriatic acid, and protoxide of mercury, or calomel, which may be swallowed in large doses with impunity. It has also the great advantage of being always at hand, and, there is no danger of giving it in excess. The practical rule, therefore is, that as soon as we are called to a person suspected of having taken corrosive sublimate, we should immediately make them swallow as many whites of eggs, well mixed with water, as the stomach can contain. It will immediately decompose the metallic salt remaining in the stomach, and if it excite fresh vomiting, so much the better. Along with this, blood-letting, &c. may be had recourse to, in order to overcome the inflammation already excited. Charcoal lately recommended as an antidote both of corrosive sublimate and arsenic, has no such effect, and acts only mechanically, by involving their particles, as alumine, or any such substance does.

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* Edinburgh Medical and Surgical Journal, Vol VII p. 89.

For arsenic, unfortunately, no antidote has been discovered; we must therefore content ourselves with filling the stomach with viscid and mucilaginous fluids, and exciting vomiting, combined with the antiphlogistic treatment.

Decoction of cinchona, strong tea, and astringents, in general seem to have some power against the poison of tartar emetic, but M. Orfila does not consider them as at all equivalent to emptying the stomach by tickling the throat, and renewed large draughts of warm water.

In sugar and syrup we are rather surprised to find an antidote for verdegris, and yet it seems to rest upon good grounds. M. Marcelin Duval has collected various proofs of its efficacy upon men poisoned with verdegris. In all, it allayed the pain and other alarming symptoms, and produced an immense quantity of liquid stools. M. Orfila has confirmed the conclusions drawn from these observations, by direct experiments on animals; and has also shewn that sugar exercises a chemical action on verdegris, and renders it insoluble in water.

Milk is the real antidote of muriate of tin, by which it is completely coagulated. The coagulum contains muriatic acid and oxide of tin, and is not deleterious.

Muriate of soda counteracts the corrosive effects of nitrate of silver.

Calcined magnesia answers very well as an antidote to the corrosive action of the acids, but must be used soon.

The sulphates of soda and magnesia are antidotes to the salts of lead and barytes. They form purging salts, which carry off the sulphate of lead and barytes.

Acetic acid is the best antidote against the effects of the alkalies.

We are afraid that we have trespassed upon the patience of our readers by the great length of this article; but we have been seduced into it, not only by the general excellence of the work itself, but also by our own predilection for its subject;—a subject of the greatest importance, and which, we are glad to see, is daily attracting more attention, and to the advancement of some parts of which the following valuable inaugural dissertations, recently published in this university, have contributed, either by original experiment, or judicious compilation and criticism. But, alas, the satisfaction we experience in referring to these real proofs of the abilities and industry of the pupils of this school, is largely mingled with regret, that the untimely death of the first gentleman on the list, has already blasted our expectations that he would have continued to prosecute the subject, with the same intelligence and zeal that are so conspicuous in his only production.

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PART III.

MEDICAL INTELLIGENCE.

REPORT of DISEASES treated at the PUBLIC DISPENSARY, *London, from August 31st to November 30th 1814.*

Acute Diseases.

	No. of Cases.		No. of Cases.
Tertiana	1	Enteritis	1
Febris	23	Peritonitis	2
—— infantum remittens	14	Cholera	2
Ophthalmia	1	Nephritis	1
Otalgia	1	Dysenteria	17
Cynanche tonsillaris	6	Variola confluens	2
Hydrocephalus	1	Varicella	4
Catarrhus	74	Rubeola	9
Pleuritis	2	Scarlatina anginosa	18
Peripneumonia	19	Rheumatismus acutus	7
————notha	3	Abortus	8
Pertussis	18	Convulsio	1
Carditis	1	Tetanus	1

Chronic Diseases.

	No. of cases.		No. of cases.
Vertigo	8	Enterodynia	14
Cephalæa	26	Colica pictonum	1
Asthma	3	Diarrhœa	17
Catarrhus chronicus	37	Peritonitis chron.	4
Hæmoptysis	10	Physconia	2
Phthisis	18	Pulsatio scrobiculi	1
Palpitatio post febrem	1	Tænia	3
Dysphagia	1	Lumbricus	1
Dyspepsia	16	Marasmus	5
Vomitus	6	Nephralgia	4
Gastrodynia	28	Dysuria	ly to
Hæmatemesis	1	Hæmaturia	crust
Pyrosis	4	Enuresis	some
Hepatalgia	1	Menorrhagia	knee
Icterus	4	Amenorrhœa	ined,

CHRONIC DISEASES continued.

	No. of cases.		No. of cases.
Hysteralgia	7	Struma	1
Leucorrhœa	7	Prurigo	6
Rheumatismus chronicus	34	Lichen agrius	2
Lumbago et sciatica	3	Psoriasis inveterata	2
Tremores post rheumatismum	1	Lepra	1
Hypochondriasis	1	Urticaria	4
Asthenia	15	Erythema	2
Hysteria	3	Purpura	1
Epilepsia	2	Impetigo	2
Hemiplegia	6	Porrigo	11
Paraplegia	1	Ecthyma	1
Hydrops	1	Scabies	8
Ascites	2	Herpes	1
Hydrothorax	1	Rupia	2
Anasarca	5	Aphtha p. variolam	1
Phlegmatia dolens	1		

The temperature of the autumn was moderate and regular; and November has been, on the whole, less foggy than usual: the atmosphere, however, was frequently loaded, and in the third week, a thick fog occurred for two or three days, accompanied by frost.

The pulmonary diseases, which usually multiply rapidly in the humid atmosphere of November, have assumed a considerable degree of severity; for, in addition to the number of cases of acute pneumonia, many of those, which are put down under the head of catarrh, and chronic catarrh, were marked by symptoms of considerable violence, especially in those persons who are habituated to annual returns of winter-cough. In several of these, the habitual attack put on the character of peripneumony; and in some, particularly in those who had not applied for early assistance, or in whom the lancet had not been resorted to, it proved rapidly fatal. The apprehensions, which many cautious practitioners have entertained, and with which we have most of us been more or less impressed, in respect to the danger of blood-letting in London constitutions, are productive probably of some mal-practice. If the debilitating effect be really great, then, it is probable, the remedial effect is more easily obtained; and appears to be the fact: for a single bleeding will often arrest monic inflammation, of moderate violence, in a person of ent vigour, and even advanced in life, sometimes during wing of the blood. Although, therefore, copious and re- d bleeding be seldom necessary, and that only in vigorous habits; yet in these patients, of a certain degree of feebleness, the opposite inference, that *no* bleeding is requisite, will be much more fatal in its consequences than any possible debility that can ensue. In cases of delicacy, however, the quantity should be determined, not by prescribed ounces, but by watching the operation, and observing its effects on the pulse and the symptoms as it proceeds. There appears to be no sound reasoning for doubting, that any constitution, which has vigour enough to be capable of acute inflammation in any important viscus, is capable also of bearing the quantity of debility (if we may so speak) which is necessary for subduing it.

The measles and scarlet fever have been very prevalent, and the former, in a few cases, were attended with considerable affections of the chest. The scarlatina has been, in all the cases, accompanied by sore throat; in the adults, indeed, in two or three families, the throat, as is usual, was the only seat of the disease, as the rash did not appear on the skin. Under the cool treatment, which, however, is often very imperfectly accomplished in the close and crowded apartments of the poor, these cases have uniformly done well. The fevers, of which twenty-three cases are mentioned, were of various types, and commonly mild and of short duration, depending apparently upon gastric derangement, and such as might be denominated Synochus biliosa. In two cases, however, a more serious affection of the bowels occurred, in the course of their progress, in which a copious and repeated hæmorrhagy took place, with pains in the abdomen, but with little or no tenesmus. The tongue and mouth, in these instances, were coated with a black fur, the pulse was exceedingly frequent and small, and much emaciation ensued. Purging, though gentle and moderated by the combination of opiates, (which succeeds in dysentery), appeared to aggravate the symptoms. Both the patients lingered a considerable time, with much moaning and expressions of distress; but with bright countenances, and only an occasional confusion of intellect; and one of them ultimately recovered, under the use of saline medicines, combined with anodynes.

The case of tetanus proved fatal, as is but too common. I had no opportunity indeed of employing any means of relief until the fifth day from the attack, when complete opisthotonos had existed for nearly three days. The disease occurred in a woman of about 30 years of age, and appeared to be ascribable solely to the irritation, occasioned by scratching off a small scabby crust from a small superficial ulceration, that had subsisted for some time below the patella on both knees. Those on the right knee were completely healed, and on the left the little scab remained, and was forcibly abraded by accident. On the Tuesday, two or three days after this abrasion, she complained of stiffness about the jaw and back of the neck; the rigidity in these parts increased, and had extended, on the following day, to the extremities, which on the Thursday became spasmodically extended, and immoveable by the will. The jaw was now locked, so that no sustenance could be introduced. On the Saturday afternoon, I saw her, and found her lying on her back, with the arms and legs rigidly extended, and the toes pointed forward. She spoke in quick, and irregular exclamations, appearing to be interrupted by the spasms of the diaphragm, after every two or three words. When asked how she was, she rapidly uttered, with catches in the breath, the words "Stop—I'll tell you—let me get —my breath." She complained of the most agonizing pain, both in the limbs and in the region of the diaphragm: the skin was hot and she perspired profusely; the pulse was about 100; and she had had no evacuation from the bowels since the commencement of the attack. The pain of such a malady may be conceived, indeed, to be excruciating, if it be considered that it is an universal cramp; and she appeared to be in torture. It was vain to attempt the introduction of any medicine into the stomach; for not even a broken tooth afforded the means of passing any thing into the throat. As the only case of tetanus, which I have ever seen terminate favourably, was treated by the cold affusion, and as this was almost the only remedy to the administration of which there was no actual impediment, a strong impression of the natural mortality of the disease, which must probably prove fatal in a day or two, was made upon the minds of the family, and the cold affusion was fully employed, at intervals of three or four hours, by dashing a bucket of water over the naked body, held in a tub. A turpentine clyster was likewise administered, but without any effect. On the following day, (Sunday,) she was materially relieved in respect to the pain, and the rigidity of the spasms, and bent the knees considerably, to shew me the command which she had regained over the muscles of the extremities; and she had none of those paroxysms, during which the body was in a great measure supported by the heels and the occiput. She still spoke with a catch, and in broken sentences; the jaw remained firmly locked; the pulse was above 100; the skin hot, and the sweating profuse; and no motion had passed. She was anxious to have the affusion continued, in consequence of the relief of pain which it had procured; and, without any consideration of the sweating surface, it was repeated. The enema was also increased in activity. But, in the course of the night, the vital powers began to sink, and she died in the forenoon of Monday, the seventh day.

The evidence of this case, though not strongly in favour of the cold affusion, is certainly not any disparagement of it. It produced, even at the late period at which it was resorted to, a considerable alleviation of the spasms, and much ease to the patient; and, had it been employed earlier, such relief might have been more effectual. I once had the satisfaction of seeing this remedy remove the disease, and the patient affirmed, that, at each application of the affusion, he gained a manifest step in the recovery of his muscular freedom. Dr G. Currey has recently witnessed the same success from the same remedy, in a case which was much less rapid in its progress, than the present one, and which he had an opportunity of treating from its commencement. * With these intimations in its favour, and in the difficulty, which the disease occasions, of administering any internal remedy, if we were in possession of it, the cold affusion seems to be the first resort of which the practitioner would avail himself in this formidable and distressing malady. T. B.

Dr Wilson Philip of Worcester was last summer engaged in a set of experiments, an account of which will soon be laid before the public; made with a view to ascertain the principle on which the action of the heart depends, and the relation which subsists between that organ and the nervous system.

From these experiments, we are informed, it appears, that M. le Gallois was deceived in his inferences from the experiments repeated by him before a committee of the National Institute at Paris, and published under its sanction.

The following, we understand, are the principal inferences afforded by Dr W. Philip's experiments.

When the power of the muscles of voluntary motion is destroyed, in order that their action may not disturb the motion of the heart, this organ is distinctly seen to obey the power of both stimulants and sedatives, applied either to the brain or spinal marrow.

∴ The heart obeys the same laws with the muscles of voluntary motion, the only difference between them arising from their being fitted to obey different stimulants. The power of both is independent of the nervous system, yet capable of being influenced through it, and that to such a degree as to be nearly destroyed by great injuries of this system. Hence the brain and spinal marrow may be removed, without impairing the excitability of any muscle; but the excitability of all may be impaired by powerful agents applied to these organs.

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* Medical Transactions of the College of Phys. Vol. IV. Art. xiii.

The circumstance which misled M. le Gallois appears to have been, that, in his experiments, he *removed* the brain, but *crushed* the spinal marrow, by forcing into the spine a stilet of exactly the same size with its cavity. When Dr W. Philip reversed the experiment, the result was reversed. Both in warm and cold blooded animals, he removed the spinal marrow without affecting the action of the heart; but found its power almost destroyed by crushing the brain. It is evident that these conclusions, if the experiments on which they rest are correct, tend to establish laws of great importance in the animal economy, which have hitherto escaped physiologists. By comparing his own experiments with those of former writers, Dr W. Philip, we are informed, has been led to the following conclusion, That in the more perfect animals, there are three vital powers, having an existence not immediately dependent on each other, the sensorial, the nervous, and the muscular powers; yet so connected, that the muscular is influenced through the nervous system, and both of these through the sensorial system.

Extract of a Letter from Dr Halliday, containing an instance of singular Recovery from a Gun-shot Wound.

By inserting the following case in the next number of your valuable Journal, you will give publicity to one of the most extraordinary recoveries from a gun-shot wound, that perhaps ever occurred in the annals of Surgery.

Thomas Watt, a private in the third battalion of royal artillery, while employed in the batteries before Antwerp, on the 4th of February last, was struck by a twenty-four pound shot, on the left breast, which completely separated the pectoral muscle from the ribs, shattering the clavicle, first rib, head of the humerus, and left scapula. When brought to the field hospital, he was in a comatose state, and the wound was considered fatal. In the presence of Mr Draper, surgeon to the forces, and Mr Samuel Cooper, compiler of that most valuable work, the Medical and Surgical Dictionary, Mr Curtis, surgeon of the first guards, and a number of medical officers, I removed the broken fragments of the clavicle, the whole of the first rib, and the greater part of the head of the humerus. It was my intention to have cut down upon the bone, and to have removed the whole of the fractured part of the humerus, but as this seemed so hopeless, and the patient appeared sinking, I contented myself with removing the loose pieces of the scapula that were found in the wound; and, after removing the extravasated blood from the *surface of the lungs and pericardium*, the fleshy parts were brought together, and kept in their position by means of stitches with a needle and ligature. The patient was carried from the operation room to a low ward, where, being covered over with a blanket and a quantity of hay, he began in a short time to revive considerably. Having taken a small quantity of warm wine, he said he felt tolerably comfortable, and expressed a wish to be carried to the quarters of his company. On the morning of the 5th, he eat a hearty breakfast of bread and wine, which was all we had to give him. Some fresh dressings were applied over the surface of the wound, but, as we still considered his recovery hopeless, we thought it would only be torturing his last moments to send him to the rear. On the sixth, however, when it was understood the army would retire, as he continued in good spirits, and the wound seemed healthy, he was put into a spring-waggon, and sent off to the General Hospital at Grootzundert; from thence he was forwarded to Williamstadt. To prevent any displacement of the parts, as well as to support the ligatures, a broad bandage was passed several times round his body, in which the wounded arm was included; and in this state he arrived at the last mentioned place, on the 7th of February, after undergoing the fatigues of a journey of not less than fifty miles. When I visited the General Hospital at Williamstadt, in the end of March, I found him in a forward state of convalescence; and having requested my young friend Mr Garret, hospital-assistant to the forces, under whose particular charge he was, to keep notes of his case, I enclose you that gentleman's communication, as I consider Watt's recovery, in a very great measure, owing to the unremitted attention and abilities of this young medical officer. In August last, I saw this patient at Antwerp, perfectly recovered; and believe he has now gone to England as an invalid. He had perfect use of his arm from the elbow-joint, but of course was unable to bear any weight, or any exertion with it.

Bruges, Flanders, 1*st December* 1814.

Extract of a Letter from Mr Edmondston on the definition of Hydrophobia.

Permit me to request, through the medium of your valuable and interesting work, that either yourselves or some of your intelligent correspondents would favour the world with a definition of the disease known by the name of *Hydrophobia.*

It is unnecessary to point out the impropriety of the name, or the defective and unsatisfactory definitions which have been given of the disease by all nosologists. While we remain so profoundly ignorant of the pathology of the disease, we shall never be able to obtain for it a name sufficiently appropriate and expressive. This, however, is not a matter of very great importance. A name may either have in itself meaning, as happens in the words hepatitis, gastritis; or it may stand as the mere symbol of certain ideas. While all entertain the same notions of certain phenomena, it is of little moment whether the name be expressive of these phenomena, or merely recall to the mind the idea of the thing signified. The word hydrophobia, therefore, as long as it only serves to present to the mind the notion of that peculiar combination of symptoms which succeeds the bite of a rabious animal, and without conveying, at the same time, any idea of its own particular derivation and import, may, until a better be found out, be deemed as suitable a word for the purpose as any other.

The definition, however, of a disease, is of more importance, as it comprehends the phenomena themselves, and professes to give us the most clear and precise notions respecting them. In order, therefore, that a definition may be concise, luminous, and accurate, it is indispensably necessary that we should have a thorough conception of the subject or thing to be defined. It is superfluous to observe, that, with respect to hydrophobia, this knowledge is altogether wanting. It is consequently impossible to define scientifically what that disease is which is designated by the title hydrophobia, because we have not terms or synonyms of a more simple and intelligible kind, into which the description of it can be resolved, and this, for the best of all reasons, that we do not possess those ideas of its nature which are requisite to guide us in the selection of the expressions best calculated to convey the clearest notions respecting the disease. Thus situated, the only resource, in the absence of a better definition, is to give a summary of those leading symptoms or phenomena by which the disease is in general characterized; leaving it to future investigation to discover and determine precisely in what these phenomena consist.

Suppose, then, we should hazard the following as a definition of hydrophobia:

"Post rabiosi animalis morsum, pyrexia levis—pulsus debilis et parvus—dolor vehemens in epigastrio—suspiria profunda gravissima, convulsiva—anxietas, timiditas, interdum iracundia—ructus, sitis sævissima—deglutitio maxime difficilis, suffocationis sensu comitata, et liquidorum horrorem gignens—initio, lingua et fauces internæ sine humiditate solita; progressu et versus finem, ptyalismus, pervigilium, plerumque cum lucis et aeris motus intolerantia."

I am aware that one obvious objection, independent of many others that may probably be made, to such a definition as this, is its great length. Yet it is not wholly without precedent in this respect. The nosological systems of Cullen, and particularly of Sagar, (*parva componere magnis*) contain examples equally objectionable in point of diffuseness. Indeed, this defect can hardly be avoided, knowing so little as we do of those changes in the condition of the system on which the morbid state depends.

I have particularly specified the dryness, or rather the less degree of humidity than usual of the mouth and fauces, in the beginning of the disease; and have contrasted it with the salivation which afterwards occurs in the course and towards the termination of the disorder. This I have done, as, from what I have seen, the accession of the salivary discharge appears to characterize a distinct and highly formidable stage of hydrophobia. I need not trouble you or your readers with any farther comments on this attempted definition of mine, but shall leave it open to your and their candid criticism, in the expectation, that, having passed through so purifying an ordeal, it will come out free from all imperfections, and benefited by such alterations and additions as it cannot fail to receive.

It is with real pleasure that we contribute to the circulation of the following active and judicious measure of the National Vaccine Establishment.

National Vaccine Establishment.

The Board of the National Vaccine Establishment, under the authority of Government—in pursuance of their duty, to extend, by all possible means, the practice of Vaccination, and, thereby, to accelerate the extermination of Small-Pox from this nation, as happily has already been effected in some countries, where, before the introduction of Vaccination, that disease was prevalent and generally destructive—do recommend to the inhabitants of parishes in the United Kingdom and its dependencies, not in the vicinity of the Metropolis, to appoint so many Members of the Royal College of Surgeons in London as may be judged expedient, *Corresponding Vaccinators* of the National Vaccine Establishment: who shall, each, be expected to Vaccinate gratuitously, the infant-poor, and other proper objects, inhabitants of the district to which he shall be appointed: to keep a register of the persons whom he shall Vaccinate, with remarks upon interesting occurrences; and, annually, to communicate a summary thereof to the Board of the Establishment, addressed to "Dr Hervey, National Vaccine Establishment, Leicester-Square," under cover, to "The Right Honourable the Secretary of State for the Home Department, Whitehall."

And every person who shall be appointed as herein is recommended, will be recognised by the Board as a *Corresponding Vaccinator* of the National Vaccine Establishment; and will, accordingly, receive free of expence, the printed directions of the Board relating to Vaccination; and, at all times, necessary supplies of Vaccine Lymph.

The Board, also, earnestly solicit the enlightened and benevolent Clergy of the United Kingdom and its dependencies, to exhort their parishioners, favourably to receive, and justly to estimate, the blessing providentially pointed out, for immediate security against the infection of Small,Pox, and, eventually, for the total eradication of that contagious, loathsome, and fatal disease; and particularly, to impress upon their minds, the special duty of the Directors of Institutions for the benefit of infant-poor, to obtain for the objects of their charitable attention, timely and proper Vaccination.

And the Board solemnly call upon all persons, especially the Magistrates of the United Kingdom, as they value the lives of human beings, to use their influence and authority in preventing the exposure of persons having upon them Small-Pox: and, so far in arresting the propagation, and forwarding the extinction of that pestilential malady.

John Latham, M. D.
President of the Royal
College of Physicians.
Henry Ainslie, M. D
James Haworth, M. D.
Thomas Hume, M. D.
H. J. Cholmeley, M.D. } Censors.

William Blizard, Master
of the Royal College of
Surgeons.
Henry Cline,
William Norris, } Governors.

James Harvey, M. D. Registrar.

Board-Room, Leicester-Square, October 6, 1814.

Lectures on the Eye and Ear.

Mr Stevenson, will commence his Annual course of Lectures on the Eye and Ear, early in January next.

In a series of twenty-four Lectures, will be given a full anatomical description and demonstration, of every part of the complex structure of the organs of sight, and hearing; an explanation of the manner in which they perform their respective functions in a state of health; and an elucidation of the History, Symptoms, and best mode of treating their various Diseases; including an account of all requisite operations, and the improved methods of performing them.

Particulars may be known by applying to Mr Stevenson, at his House, 105 Great Russel Street, Bloomsbury Square, London.

Dr Balfour is preparing Observations on the Pathology and Cure of Rheumatism, which will appear in our next. In this paper will be detailed, several cases in which *Bandages* were employed with the greatest success, both in relieving pain, and in giving to the patients full command of the limbs affected; which results suggest to the author a new view of the proximate cause of the disease.

The following is a list of all the books connected with the profession entered at Stationers' Hall, from July 30, to October 31.

Humboldt's Personal Narrative, Vol. I. and II. 8vo.——Humboldt's Researches, Vol. I. and II. 8vo.——Thomson's Annals of Philosophy, No. 20.——Wells' Essay on Dew.——Salt's Voyage to Abyssinia.——Blackhall's Observation on Dropsies, 8vo.——Faithhorn on Liver complaints, 8vo.——Dr Rees' New Cyclopædia, Vol. XXVIII. Part ii. 4to.——Carmichael on the Venereal Disease, Part i. 4to.——Memoirs of the Caledonian Horticultural Society, No. 4, 8vo.——Transactions of the Geological Society, Vol. II. 4to.——Berzelius on Mineralogy, 8vo.——The Edinburgh Medical Journal for October.——Thomson's Pharmacopœia.——The Classes of Botany, Part 3, 4, and 5.

⁂ Communications have been received from Dr Nicoll, Dr Chisholm, Mr Duncan, Mr Fletcher, Mr Hathorn, and Mr Garret.

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PART I.

ORIGINAL COMMUNICATIONS.

I.

On the Malis Dracunculus, or Guinea-Worm. By C. CHISHOLM, M. D. F. R. S. &c. &c.

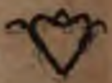
IN the introductory part of my Essay on the Malignant Pestilential Fever, I have given the outlines of the history of Dracunculus, as far as it seemed necessary to establish the fact of its being an endemic, and, during a certain portion of the year, an epidemic disease in the island of Grenada; and not, as was formerly and pretty generally believed, in the West India islands at least, always foreign, and confined to the natives of Africa imported into these islands. This short introductory notice, was, I believe, deemed satisfactory, as far as it went; but it gave rise to a wish among some medical and philosophical friends, to have that notice of this singular animal in a more detailed form. This wish was expressed immediately after the publication of the first edition of the Essay, in 1794. Since then a variety of pursuits has disengaged my attention from this object. The contrariety of opinions, however, that has, at different times since then, prevailed respecting the natural history of the dracunculus, and the pathology of the disease it produces, has of late induced me to refer to my notes on this subject; and the result, which I now beg to submit to the public, through the medium of the Edinburgh Medical and Surgical Journal, may not, perhaps, be found unworthy of their attention.

A considerable portion of my practice in Grenada lay in the district called Point-Saline, and within the limits of this district did the dracunculus appear as an endemic and epidemic disease. "This district, forming an isosceles triangle, the base of which may be four miles in length, is almost entirely composed of the soft rocky substance called Tuf; and fully a third of it is destitute of soil, except here and there in little hollows or fissures; or where the surface of tuf, softened by the action of air, has become a kind of vegetative earth. The whole of this immense mass is made up of laminæ, inclining or horizontal, as the surface is acclivous or flat. Three conical hills, of about five or six hundred perpendicular feet in height, situated in the midst of this, constituting what is called Morne Rouge, are entirely composed of vitrified rocks and stones, of a black or brown colour, and scoriæ intermixed with an earth exactly resembling iron rust reduced to powder. No trace, however, of the crater of a volcano can be discovered on any of them; but they are so placed with respect to each other, as to form a very deep circumscribed hollow, which seems to have been the crater of an immense volcano."—(Essay, Introd. p. 46.) The tuf has been the produce of this volcano, and, similar to the same substance abounding in other volcanic countries, is a compound of ferruminated ashes and decayed lava;—a circumstance of importance in an inquiry into the natural history of dracunculus. The whole of this district is destitute of natural springs or running streams of water of any description. In this district, thus singularly constituted, the negroes were necessarily restricted to the use of the water of wells dug in the tufy rock, near the inlets of the sea, and subject to the action of the tides. On some plantations, indeed, the owners had built tanks or cisterns; or had placed wooden reservoirs for the collection and preservation of rain-water; and where these were sufficiently capacious, the field-negroes were permitted to use the water they contained, in common with the white inhabitants and domestic negroes. But where such salutary accommodations had not been provided, except to the limited supply of the latter, the former invariably drank the water of the wells. On the plantations thus circumstanced, the Guinea-worm was an endemic, and, during a certain portion of the year, an epidemic disease. For the purpose of obtaining every possible information on this curious and interesting subject, I proposed to the gentlemen conducting the largest plantations on which the disease prevailed, a set of questions; and the following is the substance of the answers given. These answers were the more valuable, as they proceeded from men actually living on the spot for many years prior to the period in question, and uninfluenced by professional theories or prejudices, and urged by motives of pressing interest, to state correctly the existing facts, in order to obtain relief from a most embarrassing and extensive calamity. Mr Templeman, of the plantations of Edmund Thornton, Esq., thus stated the circumstances relative to the Guinea-worm, under his own immediate observation, viz. that the Guinea-worm had been epidemic on the plantations under his charge, during a certain period of the year, for seven years, or from 1787 to 1794, when I left Grenada;—that the well, of the water of which the field negroes exclusively drink, had been dug nearly about the same time, or about eight years; and was dug in consequence of others more distant from the sea giving an inadequate supply of water;—that the water has been always perfectly limpid and transparent to the naked eye, and has no disagreeable smell;—that it rises during flood tide about four or five feet, and sinks as much during ebb;—that its taste is brackish, and although the water itself appear to the eye pure and clear, yet there is always a considerable accumulation of filth at the bottom, notwithstanding its being frequently cleaned out, and this filth smells very disagreeably;—that the well is situated about a hundred yards from the sea, near the centre of a small valley running obliquely from the sea into the country;—that it is cut out of a soft tuf;—that the sides and bottom of the little valley are formed of the same substance, having a thin coat of soil, in which cotton and other plants are cultivated or grow spontaneously;—that those only who drink of the water of this well have been afflicted with the Guinea-worm; the white inhabitants and domestic negroes, who always drink rain-water, have never, except in four instances, had the disease;—that the field-negroes, who drink no other water but that of this well, are universally subject to the worm, so that almost every one of about three hundred has had it, to a greater or less extent, or from one to ten worms at a time in various parts of the body, during the months of November, December, January, and February of every year;—that the annual recurrence of the disease is most regular;—that it first appears generally about the 20th of November, and before the month of March, when it usually ceases, almost every field-negro on the plantation has suffered by it;—that in January it often happens that fifty negroes are laid up at once;—that from the end of March or beginning of April, to the middle of November, not one new instance of the disease has occurred, although it sometimes happens, that the ulcers occasioned by the imperfect extraction of the worm have not entirely healed before the month of June;—that this however is very rare. Mr T. gave some remarkable instances of the disease proceeding from the drinking of the well water. A negro boy, a domestic of his own, had one year, 1793, several Guinea-worms; and as he was supposed never to have drank or made any use of this water, his case was considered as a disproving argument of the efficiency of the water in producing the disease. On a careful investigation, however, it was discovered that, during the summer of 1793, the boy had drank of the well water, but at no time before. Three infants from five to seven months old, had each a worm in one of their legs. On inquiry of their mothers, it was found, that they had, during the same summer, given them water of the well to drink.

Mr Scott, the agent of a large plantation called Grand Ance, similarly circumstanced in almost every respect to the property Mr Templeman had the charge of, favoured me with answers of precisely the same import. But here a very remarkable proof of the cause of the disease being confined to the internal use of water of wells dug in tuf, and subject to the ebb and flow of the tide, was furnished. Mr Scott having minutely inquired into the nature and cause of the worm, which often deprived him of the labour of a large portion of a very numerous gang of negroes, suspected the tuf well-water to be in fault, and, under this suspicion, had cisterns built sufficient for a large supply of rain-water, and filled up the wells. The following year, not a single instance of Guinea-worm occurred, and I understand it has never since appeared. In a letter I have lately received from Mr Templeman, dated Morne Rouge, Grenada, 20th June 1814, he thus writes to me: (I should have observed, that, on my representation, Mr Thornton had cisterns built, the happy effect of which is thus shortly stated in this letter:) " As to the Guinea-worm, none have made their appearance in any of the negroes, since about twelve months after your quitting this island; and that twelve months they were more favourable, and did not come to bad ulcers and sores, as you before saw them, nor have I heard or known since that time, now eighteen years, of any Guinea-worms being on any of the negroes in this quarter."

Most of the small islands or keys, as they are called, composing the extensive groupe of the Grenadines, are formed, for the most part, of tuf or decayed lava, and ferruminated volcanic ashes. The cultivation of the more practicable spots of these islands, yields a valuable produce in cotton and Indian corn; but they are all destitute of water, and the inhabitants depend on rain water, or water of wells dug, as those of Point-Saline, in tuf, and are subject to the action of the tides. In those instances in which the planters have attentively observed the causes of the few diseases these islands are subject to, the same phenomena of the origin, progress, and annual recurrence of the Guinea-worm, have been remarked. A very ingenious and acute observer, Mr John Campbell, proprietor of one of the largest of them, called Mystique, and of a gang of about 500 negroes, favoured me with the following interesting information, in the year 1796, at St Vincent. He told me, that, about the year 1793, he first perceived among the negroes this disease;—that never, till then, had any symptoms of it appeared, to his knowledge;—that ever since November of that year, it makes its appearance about the beginning of the winter months, and continues to prevail almost universally among the field negroes, till the month of March or April;—that the fever occasioned by the irritation of the worm, has been in many instances extremely violent, attended with delirium, and other alarming symptoms;—that in all, the inflammation has been very considerable, and productive of excessive pain;—that few recovered till the expiration of six weeks or two months;—that he has known 300 out of 500, laid up with the disease at the same time;—that the embryos or ova of the animal, are evidently contained in the water of the wells, which are affected by the changes of the tide;—that the water in taste has a singular sweetishness, is somewhat muddy in the wells, but when taken out seems clear to the eye;—that his reasons for thinking and believing that the disease is caused by drinking this water, are, 1*st*, All the field negroes, who alone are subject to the disease, drink of this water and no other. 2*d*, His domestics, who make use of rain water, and no other, never had the disease, although if it could be produced by any other cause existing in the island, they are fully as much exposed to it as the field negroes; and, 3*d*, None of the white people have ever had the disease, except in one instance, and that person once or twice, inconsiderately or ignorantly, drank of the well water; that the animalcule or embryo of the worm, does not insinuate itself under the cutis through the pores of the cuticle, for none of the negroes have ever been in the habit of bathing in the water of the wells;—that he has every reason to be assured, that the animalcules or ova are taken into the stomach with the water;—and, finally, that he has frequently examined the water with a microscope, and discovered innumerable animalcules in it, of a very uncommon shape.

All the information communicated by Mr Templeman, and most of that I received from Mr Scott, I have often verified on the spot myself. I have examined with precision the well and its water of Point Saline plantation, and the various stadia of the progress of the worm, during the epidemic season. As far as it was possible, by a tolerable microscope, by filtration and other means, I have ascertained the existence, in the water, of extremely minute and agile animalcules, of nearly the adjoined figure; and of innumerable white granulated substances, little more than perceptible, even with the magnifier I used, which I concluded, on a comparison of the circumstances I have stated, to be, the former the embryos, the latter the ova, of the dracunculi. Farther than this I found it impossible to proceed in my investigation. More perfect instruments might, no doubt, have produced more perfect evidence of the existence of the dracunculi in the animalcule and egg state.



Having these facts before me, for the most part minutely observed by myself, and confirmed by the experience of three years, during which nearly 3000 cases of the disease were under my charge, I feel much disposed to offer a speculation on the mode in which the Guinea-worm is generated, and received into and evolved in the human body. Whatever can be said on the subject, in the present state of our knowledge of it, must be considered merely as speculative, or can amount to little more than very probable conjecture. Were it possible, indeed, to procure some of the dracunculi in a perfect living state, and to watch their economy in their native medium, then the inferences I am disposed to draw from the imperfect knowledge I have acquired of that economy, might become established facts, and the mystery of their generation, of the mode of their admission into the human body, and of their growth there, might be developed. But before I proceed to this, it may be useful to state the following interesting facts.*

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* In all countries in which the dracunculus is endemic, the prevailing belief of the people is, that it proceeds from drinking water which contains the ova or the embryo of the animal. Among a variety of authorities, I may cite the following:—In Africa, the Chev. de Marchais, Voy. in Guinée, l'année 1725—1727, Tom. II. 136, says, "On en attribue la cause des vers cutanés aux eaux croupissantes et de mauvaise qualité *qu'on y (en Guinée) boit.*" Mr Park says; "The Guinea worm is likewise very common in certain places, especially at the commencement of the rainy season. The negroes attribute this disease to bad water, and allege that the people *who drink from wells* are more subject to it than those who drink from streams." Travels, p. 276. Mr

In July 1812, a very ingenious and respectable friend, Mr David Inglis, from Bombay, favoured me with the following statement:—A particular friend of his was engaged in digging a well in his garden at Bombay, the soil and rock of which seem to be similar, from the description given, to those of Point Saline. The gentleman's anxiety to have the work properly executed, induced him to oversee the workmen, and even to go down into the well, with slippers only on his feet He one day perceived a sensation of pricking in one of his feet; and some days after evident symptoms of a Guinea-worm appeared. After suffering much pain, the worm was extracted in the usual way. In this instance it seems evident, that the insect in its embryo state must have made its way into the foot through the skin. It is, therefore, a fact of great importance, in the discussion of the question relative to the mode of admission of the Guinea-worm into the human body—establishing the proof that this may be effected by the skin. Mr Inglis also informed me, that a medical gentleman of that presidency had assured him, that in the soil composing the surface of some parts of Bombay island, dracunculi have been often taken alive. These facts have been farther confirmed by Dr H. Scott from Bombay, in a communication he obligingly favoured me with on the subject. He says, "The dracunculus or Guinea-worm is common at Bombay, and all over that part of the coast of India. It becomes endemic in the rainy season, and hardly appears during the dry weather, especially towards the conclusion. There are no appearances of a volcanic kind in Bombay, where it is chiefly prevalent, nor in Salsette, *although, in that island, there are proofs that, in particular parts, the mineral kingdom had been subjected to great heat.* Our strata are all of the secondary kind. Those worms are sometimes found in India, in the moist earth, in great numbers together; but we have not yet seen them in any prior form or state of existence to that of a worm. During our rainy season, the legs of people who walk among grass, and particularly

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Bruce says, that "This extraordinary animal only afflicts those who are in the constant habit of drinking stagnant water, whether that water is drawn out from wells, as in the kingdom of Senaar, or found by digging in the sand, where it is making its way to its proper level the sea, after falling down the side of mountains after the tropical rains." Travels, Vol, III. 4to, p. 37. In Asia, we have already the authority of M. Dubois.—At Ormus, an anonymous writer in the Ph. Trans. abridged, Vol. III. 138. says the Guinea-worms are bred by the water. Kæmpfer says the same thing. Freind's Hist. of Phys. Vol. I. 49. In St Domingo, M. Chevalier says, "ces vers sont engendrés dans le corps par les mauvaises eaux," &c. Lettres à M. de Jean sur les Maladies de St Domingue.

those of gardeners, are full of them. The Indians wear no covering on their legs. Those who are obliged to wet themselves frequently, are at all seasons liable to them. Men who carry water on their backs, in leathern bags, have that part of their back which is so often wetted, very full of those worms."

On comparing these facts, with those which I myself have witnessed at Grenada, there seems to be sufficient reason to believe, 1. That the dracunculus propagates by ova, or that it is oviparous, not viviparous. 2. That the insect affects an argillaceous soil, or one composed of the ferruminated ashes and decayed lava of volcanoes, and more especially when such soil has a considerable impregnation of salt, or when it is percolated by sea-water. 3. That the insect may deposit its ova in this soil, or in the interstices of the skin of the human body, when these are conveniently disposed for their reception. 4. That water percolating through such soil may have the ova floating in it. 5. That when this water becomes the drink of any part of the human race, the ova are necessarily thereby received into the human stomach. 6. That therefore it seems a just inference that the hatching of these ova may take place in the soil and in the human body. And, 7. That in the first instance the insect may in its embryo worm state insinuate itself through the interstices of the skin; in the second may be deposited in its egg state by the secretory organs under the skin, or in the interstices of the muscles, and there hatched.

In order to explain the variety which appears in the foregoing extract, it may be useful to advert to the species of this worm which more directly comes under the designation of that infesting the human body, commonly known by the name of Guinea-worm. Sauvages includes, under the generic name Malis, a number of insects which certainly have no relation to each other. Had the name been confined to those which the word μαλις properly applies to, jumentorum boum pecorumque morbi, it might have been more correct. Linnæus, no doubt aware of this, uses the generic name Gordius, and confines it to those insects which have a natural affinity to each other. He gives five species:—1. the aquaticus; 2. the argillaceus; 3. the medinensis; 4. the marinus; and, 5. the lacustris. The three first are undoubtedly frequently confounded or mistaken for each other. They indeed equally infest the human body when it is exposed to them in the situations they peculiarly affect; but in their origin, and in their specific character, they may be readily distinguished. Barbut adopts the division of Linnæus, and gives coloured figures of them, distinctly shewing the difference. The first is thus distinguished, "pallidus, extremitatibus nigris;" the second "flavescens, extremitatibus concoloribus;" and the third "totus pallidus." The specimens of the third, which I had the honour of sending to Dr Monro, senior, in the year 1794, will manifest that they were the true Guinea-worm. When Barbut says that the first is that which in Guinea and in some other of the hot countries gets into the flesh of the natives and occasions great mischief, I am inclined to think he is misinformed. The second, or clay worm, I apprehend is that which is found so frequently at Bombay in the clay. But by far the most common in hot countries is the true Guinea-worm, the third or medinensis. Had a description of those spoke of by Mr Inglis and Mr Scott been given, the point, as it relates to Bombay, might have been ascertained.

The generation of worms is very imperfectly known. Earth worms certainly copulate:—I have witnessed them in the act of doing so many times. I believe they are oviparous. Intestinal lumbrici are certainly oviparous; and there is much reason for believing that they copulate also; for the intussusceptio which sometimes proves fatal to young subjects, is occasioned by knots of lumbrici united to each other in the congressional act. I am induced to form this opinion by some instructive cases which occurred to me in Grenada. The first of these took place in the year 1787, of which notes were taken at the time. The subject was a negro child about three years old. To the usual symptoms of intussusceptio, were added most violent spasmodic twitchings of the muscles of all the body, terminating in complete hemiplegia, and inability to speak or swallow. Two hours after death, I opened the abdomen, and, as I suspected, found the small intestines filled with lumbrici. But what chiefly engaged my attention and surprise, was to find intussusceptions in seven different parts of the ileum and jejunum; and wherever these were formed, to find them caused by large knots or bundles of worms, which carried the gut from above downwards. In one of these the gut had thus doubled down at least four inches, and the doubled part was considerably thickened and inflamed. My surprise was still more excited on examining the knots or bandles, to find that their union in this state was occasioned by their being copulated or adherent in the manner I had seen earth worms *in congressus actu.* They did not appear possessed of the acute sense which the earth worms so remarkably exhibit in this state, but permitted themselves to be drawn from each other. Perhaps dracunculi may propagate in the same manner. They are indeed much more imperfect animals than lumbrici; but nevertheless I cannot accede to the opinion sometimes embraced, that their generation is equivocal, or their propagation effected by offsets or cuttings. My belief is thus expressed by an elegant writer in the Amœnitates Academicæ: "Ne quis tamen inde neget primum in naturæ scientia axioma, *quod omne vivum ex ovo*, quamvis et Hydra et Gordius in centenas partes divisi renascantur in totidem animalia." (De Memorabilibus in Insectis, Tom. II. p. 401.) The ova, then, of the dracunculi being deposited in the water they peculiarly affect, are there hatched at the appointed season. All oviparous insects, we know, deposit their ova when they have acquired their perfect state; and these ova remain unhatched until the return of the season, when the heat necessary for their evolution has been established. The dracunculi, there is reason to believe, deposit their ova about the middle of spring in warm climates; and they are evolved about the middle or end of autumn. During the intermediate time the worms seem to cease to exist; for it appears from the preceding histories that they almost never appear from the middle of March to the middle of November. The embryo worms may insinuate themselves through the interstices of the cuticle under the cutis, and there gradually extend themselves into their full growth; when, by the irritation of their motions in extricating themselves from confinement, they produce a very troublesome and sometimes a fatal disease. But in the great number of instances I have witnessed, and, as far as was possible at the time, investigated, the ova seem to have been received into the stomach with the water in which they are suspended. They pass with the nutriment into the intestinal canal, are taken up with the chyle by the lacteals, and conveyed into the thoracic duct, from whence they are poured, with the fluid in which they thus circulate, into the blood, and are finally deposited in the cellular membrane, or more generally in the interstices of the muscles, and here, as in their more natural nidus, occupy the same period of what may be called incubation. The young dracunculi proceed through the stadia of their growth, as they would have done had they entered in the worm state through the skin. An obvious objection may be made to this speculation, viz. if the eggs of the dracunculi are thus received into the human system, and are thus evolved in it, how does it happen, that, of so many received into the stomach, so small a number arrive at maturity? The answer, I apprehend, is as obvious as the objection: the action of the gastric fluid,—the imperviousness of the mouths of the lacteals to any but the smallest,—the involution of many in the excrementitious part of the food,—the acrimonious nature of the bile,—all contribute to the destruction of myriads of the eggs, in the course of their progress. Another objection may be founded on the circumstance of the insect being generally or more frequently extruded from the lower extremities. This, however, is divested of much weight, when it is admitted that the insect in the embryo state often insinuates itself through the skin; and secondly, when the structure of the lymphatic system is adverted to, the capacity of its vessels not permitting the return of the ova, once deposited in the cellular membrane, into the circulation by absorption. In general, with respect to objections which may be proposed, it may be observed, that all substances endowed with life, and even the essential oils of animals and vegetables, are not soluble in the human stomach. But the oils, although they are incapable of solution in, are capable of mixture with the chyle, and pass into the circulation with it. Many familiar proofs of this exist among animals. If, therefore, the globules of oil enter into the circulating mass, and become even a part of the secretions, milk, urine, and perspirable matter more especially, of animals, it does not require much stretch of faith, to give admission to the fact, that the ova of certain insects, although indigestible, from their vitality, in the human stomach, mix with the chyle without losing their vitality, and with it enter into the blood, and are deposited in various organs, according to the distribution of the vessels which circulate them. Many of these ova are more minute than the globules of oil, and therefore can experience no more obstruction than *they* do in their passage from the stomach to the organ or part in which they are deposited. In truth, were we called upon to explain the manner in which worms find their way into the teeth, the eyes, the urine, the liver, the pericardium, the kidneys, &c. in no other way could we rationally account for the phenomenon, than by saying that the ova of these worms are received into the stomach with the food or drink, mix with the chyle and enter into the blood in a living state, and are deposited in various organs, where they are evolved, and produce living insects. We can in no other way explain the surprising facts of worms found in the eggs of fowls, and in the intestines of a human fœtus before birth. We must believe that their ova have passed through the circulating vessels of the mother, and by this means been conveyed to the fœtus. The capacity of the stomach during life, to resist the action of the gastric fluid, but the loss of this capacity with the loss of life, is a satisfactory enough proof that substances endowed with vitality, may exist in the stomach without suffering by the action of the gastric fluid. Another analogical proof equally satisfying, and infinitely more surprising, as it relates to the pertinacity with which the ova of insects maintain their vitality, may be drawn from the fact that the most intense heat does not deprive them of it. (See Good's Anniversary Oration on the Structure and Physiology of Plants and Animals, p. 30—34, an. 1803.) The Phthiriasis interna, is infinitely more singular in its origin than the dracunculus; and, indeed, if we may rely on the account given of a case of it by Sauvages, from the information of a friend, it is a disease that throws much light on that part of physiology which relates to the ova of animals circulating through the human system, and at length hatching and giving birth under the skin to a diversity of insects. "Illi ægro anno 1728 et 1729, quotidie prodibant ex oculis, auribus, podice, urethrâ, vulnere phlebotomiæ, pediculi, pulices, forficulæ, araneolæ, lumbrici, scarabæoli, quorum icones mecum communicabat amicus." (Nos. Meth. Tom. II. 603.) The very singular instances lately recorded of a fœtus found in the abdomen of two boys, one in the Bulletin de l'Ecole de Médicine de Paris, (see Med. Surg. Journal of Edin. Vol. I. 376.) the other in the first volume of the Med. Chirurg. Trans. of London, by G. W. Young, Esq., appear to me manifest proofs that neither the digestive powers of the stomach, nor the living principle of the blood, can destroy the ova of animals received into them. I am aware that the difficulties attending the explanation of this phenomenon, may render it inapplicable as a proof in the discussion before us. It may, however, be assigned to superfœtation as explicative of its origin, with as much reason as to "a radical vice in the organization of the germs," on the supposition of the phenomenon being of the nature of twins. How do the fasciolæ hepaticæ penetrate into the biliary ducts of the livers of sheep which die of the rot? (probably a species of scrofula.) Let us recollect the first axiom in the science of nature, *omne vivum ex ovo*, and the answer made to the question will doubtless be, that the eggs of this insect were received into the stomach, and, after passing through the circulating system, at length deposited in the liver. A species of this insect inhabits the stomach of certain fish, particularly the bream, and thence called by Linnæus *intestinalis*. This insect presents an instance of tenacity of life, fully as remarkable as its existence in the stomach of the fish. Linnæus thus states it: "Hanc in brama cocta mensæ apposita etiamnum vivam vidit nobilissim. Dom. Nic. Rosen, Colleg. nost. Clarissim." It is painful to see the ingenuity of such a philosopher as Dr Darwin exerted in the attempt to explain the existence of this. He supposes the existence of the fasciolæ hepaticæ to proceed from a too dilute state of the bile, and the inactivity of the absorbent vessels of the liver. But this is a conclusion without premises; for, although the theory may account for the general morbid state of the animal, in whose liver these insects are found, yet he forgets to inform us how the ova or embryo of the insect got into the liver, (Phytologia, 4to, p. 338.) Omitting the many curious facts on record which might be adduced as farther elucidations of the proposition I have stated, more especially that inserted in the New Med. and Physical Journal of London for October 1811, in which an eruption, having every distinctive mark of small-pox of the most favourable kind, was attended with a discharge of maggots not only from each pustule, on being opened, but also with the urine and feces,—a case well authenticated; and also that related by Dr Chichester of Bath, of hæmatemesis, occasioned by and attended with a discharge per anum of many hundred non-descript larvæ, extremely active and voracious, capable of living without inconvenience (" not at all affected by either,") in alcohol, port-wine, and oil; (Med. and Surg. Journal of Edin. Vol. VII. 326.)—I say, omitting these and many other facts, I shall content myself with the following very extraordinary case, communicated to me a few years ago at Bath, by a gentleman of great respectability, who witnessed himself the phenomena, and whose brother was the subject of it. When this gentleman was between fourteen and fifteen, he suddenly, and without any apparent cause, lost his appetite and strength; he became emaciated, had hectic flushings, and the hair of his head fell off. Together with these symptoms, he had uniformly a painful sensation across the stomach. In short he was considered as in a decline, and Dr Addington, the father of the present Lord Sidmouth, who attended him, put him on a milk diet, &c. In this state, he was one day, in the presence of his father and his brother, my informant, suddenly seized with violent convulsions, in which his stomach was thrown into a spasmodic state. In one of the efforts consequent upon this state, he threw up three frogs, small but lively, besides a number of tadpoles, or animals resembling tadpoles. From that moment his recovery commenced, and was soon established. The frogs were put into water, but they became languid, and seemed out of their element; they were then put into milk, in which they recovered, and continued alive for a length of time, being constantly supplied with milk. This singular narrative gave rise to inquiry into the cause. The only information on this point Mr A—— could give, was, that the patient, his brother, had been in the habit of bathing and swimming in a pond of stagnant water, which was inhabited by myriads of frogs and tadpoles; and that his friends were thence induced to attribute his disease to this, probably, as they thought, from his swallowing the ova or spawn of these animals in an impregnated state.*

Whether it has been the obscurity of the subject itself, or the imperfect observation of those who have inquired into the nature of it, or the prejudice impressed by theory unaided by research on the mind; whichever of these causes has given rise to it, certain it is, that a singular diversity of opinion has at all times prevailed respecting the nature of the dracunculus, and the manner in which it is received into the human body. Galen thus answers the question, "quid dracunculus? Dracunculus est ulcus, quod a parte propinqua ad ipsum nervi dilationem habet. Dictus autem est, quod nervus movendo corpore, in ulcus recedat, in eoque occultetur." (Galeni Definit. Med. 437.) The original idea, therefore, of this animal, as far as relates to the name, more especially, seems to have been formed from some resemblance it bears to a snake, by the Greeks called δρακων, hence δρακοντιον. The same conception of the animal was entertained by the Romans, who gave it the same name, only modified to suit their language, and hence called by them dracunculus. But the physiology of the animal seems never to have become a subject of discussion with either the one or the other. Ælius indeed, who lived upwards of 400 years after the time of Galen, (Galen A. D. 131, Ælius 5th or 6th century,) seems, according to Dr Freind, to have formed a more accurate idea of the nature of the animal, and probably from his being better acquainted with it, the flat country and the peculiar local circumstances of Mesopotamia being favourable to its production; but as to its origin and the manner in which it generates, and more especially in which it is received and propagated in the human body, he seems to have been ignorant of, or at least is silent on, (Freind's Hist. of Physic, Vol. I. 49.) Even later than the age in which Ælius flourished, or in the 7th century, the Greeks believed it to be inanimate, for Castellus informs us, (Lexic. Medicum, verb. Dracunculus,) that Paul Ægineta (lib. iv. c. 59.) non existimat animal esse, sed nervosam quandam substantiam. Castellus himself, however, does not join in this opinion,—posse tamen animal esse ratio nobis dictitat. The Arabian physicians, from the disease being endemic in their country, were enabled to make juster observations, and to acquire consequently a more perfect knowledge of it. One of the most celebrated of them, however, Avicenna, calls it a nerve, and did not treat it as a worm, but as an abscess (Freind, l. c.) Le Clerc informs us, that "Albucasis, Avenzoar et Alsavavius, font ausi mention d'une maladie *inconnue aux Grecs*, causée par un petit ver, que naît entre cuir et chair, et qui s'y promene parcourant toutes les parties du corps. Ils ont appellée cette maladie affectio Bovina." (Hist. de la Medicine, p. 777, 4to, 1723.) It is I imagine evident from this short sketch of the earliest observations on dracunculus, that even those physicians, of a modern date, who have collected records on the subject, have been not very attentive in selecting and comparing authorities, or have taken on trust what others had done before them. The instance of this, which occurs in the contrasted quotations of Freind and Le Clerc is remarkable. But perhaps the most extraordinary of all is the definition of dracunculus given by Turton in his Medical Glossary—" A worm in Guinea so called, because it is supposed to be poisonous." It seems highly probable that the illiterate natives of those countries in which dracunculus has been endemic, were the first to ascertain its animality; and that, according to their unsophisticated perceptions of its nature, the site of the sensation the animal communicated, or the tradition of some deeply impressed evil, gave corresponding denominations to it. Thus, it is a current notion among the natives of Carthagena, that when the dracunculus has, from want of care in the beginning, completed the circle, and, according to them, joined its head and its tail, the disease generally proves fatal. They firmly believe it to be a little snake, and hence called it *cobrilla*. (Ulloa, Vol. I. p. 47.) Thus, the natives of Guinea, of Juda more especially, call it *guaason d'affo*—il a mal au pied. (Chev. de Marchais Voy. à Guinée, Tom. II. p. 136.) The natives of Africa I have spoke to on the subject give it names bearing the same import;—the Kambas, *gnaalfou*, the Coromantees, *affang*, and *emvanne*. Thus, too, the Arabians and Egyptians assign its origin to the hardships inflicted by the Pharaohs of Egypt, and call it *tarenteit*, or the worm of Pharaoh; "all bad things by the Arabs being attributed to these poor kings, who seem to be looked upon by posterity as the evil genii of the country which they once governed." (Bruce's Travels, Vol. III. p. 37, 4to.)

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* Sylvius de la Boe thus briefly mentions the existence of worms in the pericardium. Vermes in pericardio reperti cor pungendo—vel humor acris mordendo palpitationis raræ causa esse possunt. Prax. Med. l. i. c. 35, 12. Also in the kidneys, Vermes inveniuntur aliquando in renibus (quod frequens est canibus) ibique substantiam ipsorum consumunt. Tract. I. Append. c. 10, 5. Dodonæus gives an instance of worms ejected from the lungs, (ex pectore propulsi). A disease attended with cough was complained of. The cause was unknown, and the remedies employed ineffectual, donec vermis ex ejus pectore una cum tussi propulsus hominem sanitati restituit. Medicinal Observat. Exemp. rara, p. 174. Riolanus gives several instances of worms existing in the heart, &c. Talis intra venas genitus in auriculam, dextram cordis confluere potest et accrescere, qui tandem cor erodat, ut sæpius observatum in dissectis cadaveribus. Enchirid. Anatom. 152. Frankfort ed. 1676. Vermes etiam in corde generantur, de quibus Salius. Memorabilis est historia Angli cujusdam cui cor erosum fuit a verme. Historiam leges apud Aurelium Severinum. Ibid. 238.

Of late, the propagation of the disease has been assigned to contagion. My excellent and respected friend Sir James Macgrigor, and other gentlemen connected with him, or residing in the East Indies, have favoured the public with very valuable information on the disease of which dracunculi are the cause; but they have not been sufficiently communicative on the very interesting subject of the physiology of these animals; and perhaps, from not having sufficient opportunity to examine and inquire into the local circumstances of the country from which this cause of disease was derived, and deceived by the extraordinary symptoms it exhibited, which the previous knowledge I have alluded to would have explained, they have assigned it to contagion, or that principle which communicates the disease, by the emanation of its virus from the person suffering under it to a healthy one. How this can be effected in the case of dracunculi, I can by no means conceive. There is, however, I apprehend, a deficiency in the statement of the circumstances which led my friend to the opinion that the disease was infectious. He says, "During the voyage, 199 cases had appeared from 360 men of the 88th regiment; and several cases occurred among the ship's crew; but not a single case occurred among the artillery. This struck every one on board, very early, as remarkable; yet these men had the same provisions, drank the same water, and, in every other circumstance, were situated as the men of the 88th regiment, except that they were kept separate. From the outset the artillerymen were accommodated apart, on the gun-deck, in a spot divided for themselves. Not one of the officers, either of the artillery, 88th, or belonging to the ship, had the Guinea-worm. From the strong circumstances which in its course appeared, every officer on board was impressed with the opinion of its being contagious, and was inclined to use every precaution of prevention; from which, most probably, all escaped this filthy and severe disease."—(Med. Sketches, p. 208.) The deficiency I advert to is, that the water drank at Bombay Island by the detachment of the 88th, in which the disease, by the foregoing extract, almost exclusively appeared, and that which the artillery and all the officers drank at the same place, *previous to their embarkation there*, is not specified. If the soldiers of the 88th drank the common spring water, and the artillery and officers rain water, on the Island of Bombay, the exclusive affection of the former is at once accounted for. We are told that there is much evidence of the volcanic influence in the formation of Bombay and the adjacent islands;—the nature of their rock and soil—the singular caves they are distinguished by—their very disjointed appearance—all manifest a disruption from the continent, *vi quondam et vasta convulsa ruina.* We are told, also, that the soil is sterile, and incapable of improvement; that the island of Bombay, more especially, has no good water upon it; that the best is rain-water preserved in cisterns, that which their wells furnish having a brackish, disagreeable taste; and so well aware are the Gentoo inhabitants of its possessing noxious qualities, that they never make use of it,—a principal cause, it is believed, of their exemption from the *narkoo* or Guinea-worm. Gentlemen who have long resided at Bombay have favoured me with these particulars; and, from the general tenor of my reading and conversation on the subject, I have no doubt of the facts. (See Mod. Univers. Hist. Vol. X. p. 226. Encyclop. Britan. art. Bombay.) An examination of the narrative Sir James Macgrigor has favoured us with in the Medical and Surgical Journal of Edinburgh, Vol. I. will, I think, further elucidate the opinion I have formed, and render that of contagion totally unnecessary. It appears from his journal, (p. 270, &c.) that the first case of Guinea-worm occurred in the 88th regiment in September 1800 at Coulabah; that the second case occurred in October; that, on the 8th of this month, the regiment was moved to the fort of Bombay, which is said to be partly surrounded by a wet ditch; that two cases appeared in November; that the regiment sailed from Bombay on the 9th December, and arrived at Point de Galle, off the Island of Ceylon, on the 29th, a situation high and healthy; that six cases occurred during this voyage; that, in the month of February, only two cases appeared in eight companies of the regiment in their voyage along the coast of Malabar; that, in the month of March, the same portion of the regiment being still at sea, 103 new cases of the disease appeared; that, during the month of April, in the voyage from Bombay to Judda, on the coast of the Red Sea, 160 cases were in the sick-list of the disease; that, in May, only eight new cases occurred; and that, in this month, the Indian army landed at Kosseir. The obvious inference from all these premises, I imagine, is, either that the embryos of the worm penetrated through the skin, and lodged under it, or, what is infinitely more probable, that the ova of the dracunculi were received into the stomachs of the men at Bombay with the water which they drank;

that they mixed with the chyle, and entered into the blood in the living state, and finally were deposited in the cellular membrane and interstices of the muscles, where they were hatched, and produced living insects. Thus the process of evolution observed the same order, and nearly the same time, as at Point Saline, stated in the preceding part of this paper. My ingenious friend has, without intending it, deduced the same result. (See Edinburgh Medical and Surgical Journal, Vol. I. p. 284.) The observation of the natives, and the remarks of Mr Dubois, a respectable and learned missionary, in his letter on this subject to Dr Anderson, the Physician-general, are immediately in point, and, by their coincidence with the history of the epidemic at Point Saline, furnish a very remarkable elucidation of my proposition, founded on that history. "When this disorder (which prevails all over this district, as well as many others in the Carnatic and Madura, to within the distance of one or two days' journey from the sea-coast) breaks out, it is sometimes epidemic; so that I have often seen villages, in which more than half the inhabitants were affected by it at the same time. Although it appears at every season of the year, *yet it more generally breaks out in the months of December, January, and February. It is then that it becomes general in many Cantons.* Admitting that water has no share in their formation, it will not be easy to explain how the inhabitants of a village, *who drink water from one well, are attacked by the disease, whilst the inhabitants at the distance of only half a mile, who drink water from another well, are not exposed to it*; or how it happens that those living on the shores of the Cavary, and other rivers, who constantly drink their limpid waters, are never visited by it; whilst those who live at the distance of one mile on both sides, and *are obliged to drink the saltish water of wells, are all, or the most part, yearly exposed to it.*" Dr Anderson, although desirous of maintaining the admission of the worm by the skin, cannot get rid of Mr Dubois's powerful fact. "Nor can we cavil," says he, "with the idea of an entrance by the chylopoetic organs, when we see worms in the eyes of horses, and the livers of sheep." (Edinburgh Medical and Surgical Journal, Vol. II. pp. 301—305.) Dr Anderson afterwards remarks a very singular circumstance in the history of dracunculus, which perfectly corresponds with my own observation at Grenada, and with that of others at Bombay. "Your notice of wells of saltish water, where the Guinea-worm is most frequent with you, renders it in some degree probable that vitriolic and marine salts, which are found both in wells and in the sea, are not unfavourable to their propagation."

But, setting aside these disproving facts, if they may be so considered, respecting the contagious nature of dracunculi, it is evident that the "exanthemata viva" can in no instance be infectious in the manner supposed to have happened in the 88th regiment. "Insecta ejusmodi minutissima forte acaros diversæ speciei, causas esse diversorum morborum contagiosorum, ab analogia et experientia, hactenus acquisita, facili credimus negotio; neque repugnat horum structura et magnitudo; minima enim sunt animalcula quæ oculus humanus adhuc percipere potuit. Ex sensibus agitur, ad rei impossibilitatem non est argumentandum; æque enim in minimis ac maximis sapientissimi conditoris artificium elucet." (Amœnit. Academ. Tom. V. p. 94.) In this learned and interesting discussion, the author employs the words contagion and contagious in their literal and legitimate meaning, as they are derived from the verb contingo, and not in the figurative sense in which they are generally applied. By contact, then, colonies of minute insects may emigrate to and form establishments on a healthy person from a diseased one. But such colonizations are confined to insects of the acarus kind; and in all the diseases which the writer of the Exanthemata Viva attributes to the agency of insects, different species of the acarus are alone the cause. The nature of dracunculus is inconsistent with this mode of propagation, more especially under the circumstances the soldiers of the 88th regiment are represented to have been in on shipboard. On the other hand, there is much reason for believing, as I have already said, that the ova of the dracunculi were received into the system by the stomach, and deposited in the cellular membrane, and, having attained their period of evolution, appeared in the persons of the soldiers in such numbers as to induce my respected friend to assign the disease to contagion, in the figurative sense in which the word is usually received. The period of evolution corresponds, it will be observed, precisely with the narrative Sir James Macgrigor has given, and with the history so clearly stated by M. Dubois. The very interesting account of the Acarus siro of Madeira, there called *ouçöes*, by Dr Adams, is a fine exemplification of the *contagion* of insects, or of the exanthemata viva. (See Medical and Surgical Journal of Edinburgh, Vol. III. p. 343.)

It now remains to offer my observations on the prevention and cure of the dracunculus. Never was there a disease to which the medical precept, *sublata causa, tollitur morbus,* more distinctly applies than this. The result of the application of this precept at Point Saline, in Grenada, is a manifest exemplification of the means by which this is to be effected, and precludes the necessity for saying more on the subject here. As to the cure of the disease, that is to be accomplished by the destruction of the insect. I used a variety of means, but none effectual, until I had recourse to mercury. Mildly saturating the system with this medicine destroyed the insect. I since then find that this medicine has been long used by others for this purpose. At that time, 1794, I knew of no authority for it. Linnæus says, "Infuso mercurii sublimati corrosivi Swietenii intra dies 20, qui alias intra 40 educitur." (Syst. Nat. Tom. I. p. 2. 1075.) In phthiriasis, and other diseases of the exanthemata viva, mercury has been long known as an effectual remedy externally applied; but in dracunculus it is not so: the remedy must pervade the system, in order to destroy the insect, or its ova. It is unnecessary to detail the variety of means employed in different countries. They are of doubtful effect, however vaunted of—asafœtida, garlic, the root of angelica, sulphur, &c. &c. Some gentlemen, considering the disease as an inanimate substance, recommend the extraction of it by a painful surgical operation; but the opinion is as irrational as the cure is unnecessarily cruel.

Bristol, 31st October 1814.

II.

On the Relation subsisting between the Time of the Day, and various Functions of the Human Body; and on the Manner in which the pulsations of the Heart and Arteries are affected by Muscular Exertion. By ROBERT KNOX, M.D. Edinburgh.

[Concluded from p. 65.]

SECTION II.

IN this section, we propose considering the effects of muscular motion on the pulsations of the heart and arteries. In what manner these pulsations are augmented in number, vigour, fulness, &c. by exercise, or, to speak more generally and correctly, by muscular motion, it is perhaps impossible, in the present state of our knowledge, to say. It seems however probable, that farther research may shortly lead to notions more precise than the ones we at present possess. Our concern is with the fact itself; *viz.* that, by muscular action, the pulsations of the heart and arteries are augmented in power, velocity, &c. This fact, apparently so unproductive, and, by reason of its perpetual occurrence, so little apt to excite attention, shall yet, on examination, be found to throw some light on the physiology of the human body, and may perhaps assist in exonerating it from a charge so lavishly and inconsiderately bestowed,—that it is a science destitute of fixed principles; or, at least, that a knowledge of the laws by which the animal economy is regulated, is, in a great measure, placed beyond the sphere of human intellect.

At all times it must have been observed, that muscular exertion, almost of any kind, but more especially violent exercise, increased greatly the powers of the heart and arterial system; but that this extended even to the slightest muscular motion, such, for example, as is made use of during a change of posture, does not appear to have been suspected, or if so, its importance has been greatly overlooked. We may reduce the section to the following heads:

1*mo*, The most powerful stimulant which can be applied, in order to increase the action of the heart, is exercise. Walking at the rate of four miles an hour, requires at least a pulse equal to 132 *per* minute; and the time of the day, and the continuance of the exertion, less affect the rate of pulsation than one would *a priori* imagine. When I say, that walking at such a rate requires a certain increase in the arterial pulsations, I do not mean to assert, that an equal increase must necessarily occur in every individual. These numbers are added only to render the subject more definite; in short, as a single example,—as a proof that the increase is great, even in the strong and healthy. We shall immediately see how very differently the debilitated are affected. This high excitement is not followed by proportional exhaustion, in so far as regards the arterial system; a fact singular enough, since it is at variance with certain laws of the economy, supposed to rest on a secure foundation. Other organs certainly follow a different mode, and exhibit, after any increase in their functions, signs of exhaustion or weakened energy. Such a law seems to hold with regard to the nervous system, in particular with the digestive organs and the respiratory. *

2*do*, No stimulant which I have hitherto tried, has excited the pulse so much even as moderate exercise.

3*tio*, Various observations have rendered it probable, that the increase in the number of the arterial pulsations accompanying muscular motion, is greatly influenced by the debility or weakness of the individual. Were it allowable to apply the rigorous language of calculation to a science which cannot be called *exact*, we would say that such increase is in the direct *ratio* of that debility. Hence, in fever, the slightest change of posture shall often produce an incredible velocity of pulse. Persons who have suffered much from loss of blood, or by chronic complaints, cannot bear the erect posture for any length of time; and hence, in the debilitated, who, it is well known, are very subject to faintings, the slightest muscular motion, by inducing or necessitating a rapid motion of the blood, (in them too rapid), shall give rise to that distressing accident.

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* Annals of Philosophy for November 1813, p. 328.

As exercise increases, so other stimulants diminish the frequency of the pulse in the debilitated, at least generally. But as there may be various kinds of *debility*, in each of which the pulse may be differently affected by exercise, and as this increase of the pulse, when it does take place, is often accompanied with irregularity, a very extensive series of experiments is required, before we can implicitly agree with the above rule. On this latter principle, even in its present state, we may explain, I think, satisfactorily, many of the supposed stimulant effects of foxglove, which drug debilitates, directly and greatly, most *systems* of the animal economy.

4to, The time of the day has a very considerable effect on the augmentation of the arterial pulsations by moderate exercise. As this relation has already been considered at some length, I shall here confine myself to the statement of a few general results. During the morning, the mere change of posture from the horizontal to the erect, shall increase the pulse by about 15 or 20 beats. At mid-day, this increase shall be 10; and, in the evening, 4 or 6. The effects produced by the sitting posture, assumed after the horizontal, are not half so considerable as those occasioned by the erect posture. The above is the manner in which the arterial pulsations are affected by posture, at different periods of the day; and though the results here stated may be greatly magnified in some, and equally diminished in others, it may, I think, be laid down as a general rule, that similar laws constantly regulate the healthy pulse.

From the above observation, we readily perceive, of how little consequence the details of physicians are, regarding the diseased pulse. If the slightest change of posture can in an instant excite the pulse by 50 or 60 beats, how easily may the medical man deceive himself and others! how often without a previous knowledge of these facts, may drugs seem to benefit the sick, when they are inert, or actually pernicious.

5to, The increase of the pulsations occasioned by change of posture, may shortly prove a valuable *asthenometer*. There are others, but they do not seem so certainly to indicate debility as the

the above. Some of them, indeed, are more calculated to detect disease than simple debility. These *asthenometers* are * the hot-bath, the cold-bath, or cold air, also the non-excitation of the pulse by stimulants, supposed to happen in those habituated to spirituous liquors. Some may allege, and perhaps with justice, that this shall be found to indicate rather a state of disease than debility. This condition of the arterial, or perhaps nervous system, which renders the pulse non-excitable by stimulants, is supposed to arise from other causes besides the abuse of spirituous liquors. "Une † observation très remarquable que j'ai eu occasion de faire, c'est que, lorsque la sensibilité a été émoussée par une affection chronique, les opiacées finissent par n'avoir plus de prise sur l'economié animale." I have seen one case which favoured somewhat the above opinion,—the case of a young man, who, without doubt, laboured under hereditary predisposition to consumption. His pulse, on one occasion, I found to be not at all affected by a very great quantity of spirituous liquors. The opinion remains to be proved or refuted by additional observation. Indeed, as it is customary with those labouring under chronic complaints to resort to the use of narcotics, (this practice had not been adopted in the case just mentioned) it is not to be wondered at that this class of drugs should at last cease to produce their wonted effects.

Somewhat connected with this subject is the detection of commencing disease in the lungs, or pulmonic debility. This, according to Beddoes, ‡ is indicated by a continued high pulse. "When consumption is advancing, it will be more frequent than natural, and, in general, much more frequent towards the close of day." The whole subject is novel, and deserves attention.

As my only wish in prosecuting these experiments has been to correct a few notions regarding the physiology of the human body, and to advance that estimable science, I shall feel gratified with an examination of my experiments, whether that lead to a refutation or to a confirmation of the opinions maintained throughout this essay.

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* Beddoes's Hygeia.

† Nouveaux Elémens de Therapeut. et de Mat. Med. par Alibert, Tom. II. 506.

‡ Essay on Consumption, p. 252.

III.

Observations on the Pathology and Cure of Rheumatism. By William Balfour, M. D. Edinburgh.

Tantum, quantum quisque potest, nitatur.—Cic.

Sydenham was the first who favoured mankind with a distinct and accurate history of rheumatism. Before his time it seems to have been confounded with gout; since that period it has maintained its ground as an idiopathic disease, and has for long been as well understood as any other, the proximate cause of which can be matter of conjecture only.

Concerning the proximate cause of rheumatism various opinions have been entertained. Dr Macbride and others imagine it to consist in a peculiar acrimony; others in a lentor of the fluids; and Dr Cullen supposes it to be, in the *acute* species, a phlogistic diathesis of the blood, with a peculiar affection of the muscular fibre; but, in the *chronic*, "an atony both of the blood-vessels and of the muscular fibres of the part affected, together with a degree of rigidity and contraction in the latter, such as frequently attends them in a state of atony."

That in *acute* rheumatism a phlogistic diathesis of the system prevails, admits not of doubt, and that there is an affection of either the muscular fibre, or of the cellular membrane, or of both, is equally certain. An atony, in the *chronic* species, of the muscular fibres, or of the cellular membrane, must also be admitted. But which of these it is, the muscular fibre, or cellular membrane, that, in either species of the disease, is primarily affected, is a matter not quite so manifest. To ascertain this point with any degree of clearness, a review is necessary of the structure and functions of these organs themselves; and I am not without hopes of being able to adduce facts, in the history of cases hereafter to be detailed, which render it highly probable that there is in rheumatism, *chronic* as well as *acute*, an affection of the *aponeurosis* of the muscles, and perhaps of the whole cellular substance connected with them, which forms a principal part of the proximate cause of the disease.

The cellular membrane abounds everywhere in the human body. It covers the whole, and connects every part. The celebrated Haller, indeed, considers it as constituting the greater part of the whole mass. All the blood-vessels receive a coat from

from it, from the *aorta*, where it emerges from the heart, to the minutest capillary that enters a tendon.

The nerves are composed of fasciculi, and these again of filaments or fibrillæ. The cellular membrane furnishes a sheath to every nerve, a covering to every fasciculus, and every filament is a tube of the same substance, filled with medullary matter; and on the parieties of this tube are ramified, and are supposed to secrete the medullary substance it contains, those capillaries, of exquisite minuteness, which are continuations of the arteries seen to penetrate the fasciculi. A muscle, whatever be its length, breadth, or thickness, is not a mass of homogeneous substance, but, like the nerves, is formed of many fasciculi. These fasciculi are themselves composed of fibres so delicate and numerous, as to be divisible *ad infinitum.* At all events, their minuteness is such as to have hitherto set at defiance the patient investigations of the most accurate physiologists to discover their ultimate division. But, incalculably minute as are these fibrillæ, they are supplied with vessels and nerves, and inclosed in a cellular sheath, upon which perhaps the vessels and nerves terminate. They are also connected to each other by means of cellular membrane, to form the fasciculi, which in their turn, are enveloped in a common sheath of the same substance, similar to that which covers the muscle, and separates it from all other parts.

The structure of the cellular tissue is extremely vascular. Mascagni considered its laminæ as consisting entirely of lymphatics; Ruysch, by his injections, reduced the membranes and cellular substance into a net-work purely arterial, the texture of which was so close as to leave no observable space for any other vessels. Hence he concluded, that capillary arteries formed the basis of membranous and cellular tissues. The truth, however, lies between the opinions of these two celebrated physiologists. For, if one set of vessels only are injected, they become distended, compress and conceal the neighbouring parts; and we know, that lymphatic absorption and arterial exhalation take place from all the internal surfaces, by which is proved the existence of both arteries and absorbents in membranes and cellular tissue.

From these data it may justly be inferred, that the functions of the cellular membrane are not merely mechanical; that it does not merely cover, connect, divide, suspend,—but that it serves a purpose in the animal economy, essential to the healthy functions of the parts on which it is so liberally bestowed. It is not consistent with common sense and reason to suppose, that a substance formed as is the cellular tissue of vessels, which carry on, if I may so express myself, the business of life in every part of the body, can remain sound, when the muscular fibre is affected with disease. It is infinitely more rational to conclude, that the affection of the muscular fibre is a consequence of the affection of the cellular substance, whose functions seem, from its universal diffusion, subservient to those of every other organ. For if, as we have seen, the cellular membrane is formed of capillary arteries and absorbents, the balance between their functions being in any degree overset, must produce corresponding phenomena. Thus, if the absorbents of a muscle carry off more than the arteries deposit, permanent contraction, in every direction of that muscle, must be the consequence.

It is well known, that diseased appearances are often confined to the cellular membrane, and that, when this is the case, the organs or vessels which it surrounds exhibit a morbid action. It is likewise ascertained, from ample observation and experience, and it goes far in countenancing our theory, that, however insensible the tendons and aponeuroses of muscles are in their sound state, injuries and morbid affections of these parts are accompanied with pains unusually severe. Thus, inflammation of the integuments, the subjacent cellular substance and *fascia* of the fore-arm, sometimes the consequence of blood-letting, occasion not only the most inexpressible sufferings, but not unfrequently render the arm contracted and rigid for life.

But the knowledge we possess of the functions of the cellular membrane, goes a great way in explanation of the phenomena of rheumatism. We know that it officiates at once as a *fascia*, a ligament, a mucous gland; and that by it is secreted all the fat and oily substance that is deposited about the joints, upon, between, and in the interstices of the muscles.

In the first place, then, the *fasciæ* of the muscles confine them to their situation parallel with the bones. Were that not the case, a muscle could not contract at all. Destined to act in a certain sphere, if that sphere is lessened, a muscle will either not contract, or its contraction will be of no use. Suppose, for instance, the *biceps flexor cubiti*, the *Sartorius*, any rectilineal muscle, was deprived of its natural bandages that confine it to its situation, what appearance would it exhibit in a state of contraction, were it possible to contract at all in such circumstances? A muscle in contracting, becomes shorter, thicker, harder, rough, and vibrates like a cord when put upon the stretch. It is evident, therefore, that unless a muscle were bound by its *fasciæ* or *aponeuroses*, its contractions would either be very limited, or, in contracting, it would start from its place like the string of a bow. It follows, that a perfectly sound state of the *fasciæ* of muscles is necessary to their vigorous action; and on the supposition of there being in rheumatism a morbid affection of the *aponeuroses* of the muscles, are explained the pain and difficulty of motion, in the first stages of the disease at least, of a limb affected with that disorder.

In the second place, the doctrine of a morbid affection of the *aponeuroses* of, and cellular substance connected with, the muscles, accounts satisfactorily for the debility of the latter,—their permanent contraction,—their rigidity in *chronic* rheumatism. Were it not for the cellular membrane lubricating the surface of the muscles, it is impossible they could move upon each other; they behoved to contract simultaneously, and to the same degree, otherwise inflammation and adhesion could not fail to be the consequence of their friction upon each other. If, therefore, there should be, from a morbid state of the cellular membrane, a deficiency of that lubricating substance which facilitates the motion of the muscles, rigidity and pain upon motion must in proportion be the consequence. Not only so, if there is a deficiency of the fatty substance, which, in a state of health, is found in greater or less quantity, deposited between, and in the interstices of the muscles, they must become shorter in proportion,—their fibres must approximate nearer to each other in every direction,—for the muscular fibre is not a line of continuous substance extending from origin to insertion; it is made up of many pieces whose ends are connected with each other, laterally, by means of vessels, nerves, and cellular substance. What, therefore, has hitherto been deemed a wasting of the muscular fibre, may, with greater propriety, be considered a deficiency of those cushions which give plumpness to every muscle, and symmetry to the whole. Not but that there is a wasting and contraction, or absorption of the molecules of the muscular fibre itself to a certain degree, and also an affection of the nerves of a part affected with rheumatism. For if we reflect, that the fasciculi and fibrillæ of both the nerves and muscles are enveloped in cellular membrane, we must infer, that the apposition of muscular, and the secretion of nervous substance, are performed by the capillary vessels of that membrane. If, therefore, the functions of these vessels are impaired, the parts they supply must suffer in their turn. But mere disuse will produce emaciation of a limb. There is no fact in physiology better established, than that action is necessary to circulation, and circulation to nutrition. These are, indeed, matters of daily experience. Emaciation of the muscles must therefore be considered more as a consequence than a cause of rheumatism.

In the third place, the effusions that sometimes take place into the sheaths of the tendons, is a proof that the cellular membrane is principally affected in rheumatism. I have seen effusions of this kind occur as the first symptom of the disease; and I have an instance of it in a gentleman who is presently under my care. In this case, a circumscribed tumour, without the least previous pain, appeared in the course of the tendon of the ring finger of the left hand. Unable to explain the cause, the gentleman took little or no notice of it for some days, when it became the centre of pains which shot alternately to his wrist and to the points of his fingers, with frequent and most painful catchings of the tendon on which the tumour was situated. These pains and spasms occurred chiefly in the morning when warm in bed; and in a short time, the patient became unable to grasp any thing with his hand, with any degree of firmness. When he applied to me, I desired him to grasp his left hand firmly with his right, pressing the thumb of the latter upon the *aponeurosis palmaris* of the former, and, in that situation, to try what command he had over his fingers. He could instantly use them with the utmost ease and with the greatest vigour. I next directed him to lay firm hold of the wrist of the affected hand with the other, and in this way to try the effect a bandage would produce. This gave him a more complete command still of the whole hand, and enabled him to do with it what he pleased. A clearer proof that the *annular ligament* was deficient in power, cannot be adduced. I endeavoured to persuade this gentleman to wear a bandage about his wrist, till a radical cure was obtained; but, highly amused with the success of the above experiments, and believing that he has the means of relief at command, he satisfies himself with the use of the flesh-brush, and, as occasion requires, with the repetition of these experiments, which never fail of the desired effect. Now, if we reflect that parts of similar structure, though far distant, are often observed to sympathize, we cannot wonder that the tendinous aponeurosis of the muscles, and all its processes, which are of the same nature and structure with the sheathes of the tendons, and indeed but a continuation of the same substance, should be simultaneously affected with them.

In the fourth place, from the fact that the same cause affects different parts of similar structure in different individuals, and in the same individual, perhaps, at different times, we might judge *a priori* almost, that it is not the muscular fibre, but the cellular membrane that is peculiarly affected in rheumatism. Thus, exposure to cold produces in one person catarrh, in another pneumonia, in another pleuritis, in another rheumatism, according to idiosyncrasy. Now, the parts affected in catarrh, pneumonia, pleuritis, however they may differ in density, are of the same nature with the tendinous aponeurosis of the joints and muscles, and the cellular substance interposed between their fibres. It is, therefore, not illogical to conclude, that it is the cellular membrane, not the muscular fibre, that is primarily and peculiarly affected in rheumatism. Not but that there must also be an affection of the muscular fibre; for, as has already been observed, the connection betwixt the two, the cellular substance and muscular fibre, is so intimate, that we cannot well conceive the former to be in a morbid state, and the latter remaining altogether sound. But we must repeat, that this affection of the muscular fibre is secondary, and therefore cannot be considered as constituting a proximate cause.

In the fifth place, Dr Cullen has remarked, that the affection of the muscular fibres attending rheumatism, seems to explain why sprains and spasms produce rheumatic affection. That sprains and spasms frequently terminate in rheumatism, is an acknowledged fact. But this fact is no way explicable on the supposition of a peculiar affection, in that disease, of the muscular fibre. For it is not the muscular fibre that chiefly suffers in these accidents. The muscles are capable of acting with amazing force. They sometimes rupture their natural bandages; lacerate their tendons; and even the bones themselves have been broken by the violence of their action. It is not probable, therefore, that such powerful organs will be the first to yield in such a trial of strength as takes place in sprains, between them, their tendons, and tendinous *aponeurosis.* The fact is, that in cases of sprains, patients uniformly point to the situation of some *aponeurosis*, tendon, annular, capsular, or interosseous ligament, as the seat of debility and of pain. No conclusion, therefore, can be more natural, than that rheumatic affections arising from such accidents, are not affections chiefly of the muscular fibre, but of that substance which is of the same structure with the parts originally affected.

Finally. It would appear from the history of rheumatism, that the proximate cause, of the *acute* species at least, of that disease, is an affection of the aponeurotic expansion of the tendons and membranous covering of the joints. This appears from joints being first affected with pain, and its consequences. In many instances, indeed, the pain is confined entirely to the joints. At other times the muscles come to be affected with pains shooting along their course from one joint to another. But is it by the muscular fibre that these pains are propagated? Is it not more consonant with the other phenomena of the disease, to suppose that these pains are occasioned by the oscillation of the contents of the extreme vessels? It is ascertained, that when red blood is, in consequence of inflammation, forced into vessels that do not naturally admit it, resolution is affected by that blood taking a retrograde course, till it comes to vessels of sufficient calibre to transmit it; in inflammations of the eye, for example, and of the *pleura*. In the latter, no symptoms of inflammation have appeared, upon dissection, in cases which, previous to death, exhibited every symptom of it; a circumstance that can be explained only on the supposition of the reflux of the blood, after death, from the part affected. From this view, it is more than probable, that vessels so extremely minute, and so very irritable and contractile as are the capillaries of the *aponeurosis* and cellular membrane, are so affected, in rheumatism, as to be incapable of transmitting, as in health, the colourless part of the blood, which therefore, by its oscillations and pressure against the parietes of its vessels, occasions those pains, hitherto believed to be propagated from joint to joint, by the muscular fibre.

Independent of the primary affection of the joints, the very nature of the remote causes themselves renders it probable, that the proximate cause of acute rheumatism is an affection of the membranes connected with the muscles. Sudden changes of weather, the application of cold to the body when warm, cannot but affect those parts soonest, and to the greatest degree, that are most exposed to their operation; and the parts so exposed are, next to the extreme vessels of the skin, the aponeuroses of the joints and muscles. The powerful muscles of the larger joints, the most common seat of rheumatic affections, are necessarily furnished with *fasciæ*, strong and dense, and whose vessels must, of course, be extremely minute. It is, therefore, *a priori*, to be expected, that they must suffer constriction from the application of cold to the body, sooner than vessels of less irritability and contractility, but of larger calibre.

But *chronic* is often the consequence of *acute* rheumatism. Nothing, therefore, can be more evident than that what constitutes the proximate cause of the latter must also form that of the former. For the only difference betwixt the two *species* of the disease consists in this, that the *acute* is accompanied with fever, whereas the *chronic* is free, or nearly free, from it. It necessarily follows, that, if a phlogistic diathesis of the blood, amounting to fever, and an affection of the *aponeurosis* and membranes of the joints and muscles, constitute the proximate cause of *acute* rheumatism, the *chronic* species of the disease must also acknowledge for its proximate cause, an affection, not different in nature though it may be in degree, of the same membranes and aponeuroses.

I was led into these speculations, near as they may approach, or far as they may be from the truth, by what occurred sometime ago in my own person, and which I have often since practised upon others, with almost uniformly the same good effect. Having been seized with a rheumatic affection of the left shoulder, chiefly in the course of the *deltoid* muscle, the pain at times, but especially towards morning, when warm in bed, was so severe as to make me cry out. Desirous, on one of these occasions, of moving my arm, a task to which its own powers were unequal, I grasped it firmly with my right hand, about the middle of the pained muscle. To my surprise and high gratification I was instantly relieved from pain; and while I thus held my arm, I could do any thing with it I pleased without farther aid from my right hand than mere compression. This, therefore, was the remedy, the only remedy to which, on all future occasions of the kind, I had recourse, and it never was employed without success.

I now began to think, that surely the muscular fibre was not the seat of pain in rheumatism; not even of those pains occasioned by motion. If it were, how could mere compression enable it to contract with all its pristine vigour? I observed, moreover, that when, during the paroxysms of pain, I endeavoured to move my arm, the moment the belly of the muscle began to press on the *aponeurosis*, I was obliged to stop; but as soon as artificial resistance was opposed to it, the muscle could perform its functions with the utmost ease. A more decisive proof, I think, cannot be adduced, that the pain and difficulty of motion of a limb, afflicted with *chronic* rheumatism, are not referable to the muscular fibre. It may occur to the reader as it did to me, that the sudden relief from pain which I experienced, is to be explained on the principle of ligatures interrupting the progress of pain, or any other sensation, along the course of the nerves, as in some cases of *epilepsy*, *whitlow*, &c. But this by no means accounts for the fact. For I always remained free from pain for a considerable time after compression was removed. I find, upon inquiry, it is no uncommon thing for people afflicted with rheumatism, to grasp and nibble a pained joint or muscle, for the purpose of obtaining even temporary relief, which they never fail to procure in greater or less degree.

A very pertinent instance of the effects of compression occurred to me, on the 14th of November last. A woman, twenty-seven years old, and very much emaciated, complained of a rheumatic affection in her left shoulder, which had rendered her arm next to useless for many weeks. The pain was confined to the *scapular* portion of the *deltoid* muscle. She could by no means raise her arm to a right angle with the trunk of her body. She could not put her hand to her mouth, far less touch with the points of her fingers the crown of the head. Placing her before me on a form, I made gentle pressure with the palm of my hand on the pained part, desiring her, at the same time, to raise her hand to the crown of the head. This she could not do. I increased the pressure, and the motion of the arm became in proportion free. This process was continued till she could move her arm in every direction, with little or no uneasiness. What surprised the patient most was, she in a very short time became capable of using her arm, with nearly as much freedom without, as with the pressure.

How are these facts to be explained? Thus, in my opinion. The *aponeurosis*, and perhaps the whole cellular membrane of parts affected with rheumatism, is in a state of morbid sensibility; this state of sensibility arising from a preternatural distension of its vessels. Bandages and pressure, by affording support, take off tension, and facilitate motion, which in its turn promotes circulation. The vessels being in this way unloaded, morbid sensibility is diminished; and if the bandages or pressure be often enough applied, or for a sufficient length of time, the vessels and membrane recover their tone. It is evident, therefore, that in proportion as these effects are produced, the motion of a part formerly rigid, will not only become free, but remain so. On this principle it was, that the motion of this patient's arm remained free after the removal of the pressure; on this principle is explained, the good effects of friction, &c. in rheumatism. If a person could always move a limb affected with this disease, little more would be necessary to the cure of it. But as that is, in many instances, impossible, and in all, attended with difficulty, in a greater or less degree, bandages or pressure, according to circumstances, by facilitating motion, become, if not of themselves a complete cure, at least most powerful auxiliaries, of which, whoever once makes trial, will, when circumstances require, ever after avail themselves.

I did not see this woman again till the 10th of December, (the distance of near a month from the time she called upon me, —a month too of the most horrible weather that can well be conceived,) when I called at her house to ascertain if the cure was permanent. She told me, she allowed the bandage which I applied, to remain till quite dirty, and that when she removed it to have it washed, she found she could do perfectly well without it, and in my presence performed, with the utmost facility, all the motions of which the arm in its soundest state is capable. In one word, this woman's case is an instance of a severe rheumatic affection, of many weeks standing, being immediately, completely, and permanently cured, by pressure alone.

On the 21st of October last, I was called to a girl of fourteen years of age, whom I found sitting in a warm room, before a large fire. Her skin was hot, face flushed, and pulse at ninety; all which I was disposed to attribute to the warm regimen she had been so carefully observing. I desired her to remove to a distance from the fire. She told me she could not move, on account of rheumatic pains, reaching from her ankles to the middle of her thighs. This account I soon found to be correct, for when, in consequence of my pressing her, she made an attempt to rise, she was forced to cry out most bitterly, so that I saw her limbs were totally immoveable. I told her I would make her walk through the room, without pain, before I left her. So much difficulty was experienced, however, in removing her from the place where she sat, that I began to suspect I had put my credit to too great hazard, and therefore endeavoured to pass off, as a joke, the assurance I had given of instant relief. Having accomplished the removal of the patient to a proper situation, I applied a roller of flannel, with a degree of tightness which she could easily bear, to both limbs, beginning at the middle of the thighs, and continuing it downwards over the feet. I now requested she would make an attempt to walk. To this she consented, on condition of being indulged with another person's arm. I allowed her to touch gently her aunt's shoulder. The girl rose up and walked through the room, stifly to be sure, but without complaint. I ordered her to be put to bed, and as she seemed to labour somewhat in her breathing, was desirous of taking a little blood from her arm. This was peremptorily resisted by her aunt, the lady of the house, on the score of the patient's youth. It was in vain I represented, that, though bandaging the limbs was a powerful auxiliary, it never could supersede the diligent use of other remedies generally employed in the cure of rheumatism; above all, that it could have no influence whatever on febrile symptoms. Next day the patient's general health was much the same; the pains as bad as ever. I suspected the cause, and soon found the bandages were hanging loose. I immediately replaced them with the same good effect as before. With the exception of some laxatives, this girl got no other remedy, and the pains were, notwithstanding, put to flight in a few days. A sister of this girl, aged eighteen, soon after my patient got better, was seized in like manner, all over the inferior extremities, with rheumatic pains. Without calling any medical aid, she had recourse to the *bandages,* which had the same happy effects with her as with her sister.

A lady, upwards of forty, who has for many years been a martyr to rheumatism, on hearing "I had found out a cure for it," applied to know what it was. I readily informed her that the application of *bandages,* or *pressure,* according to the situation of the parts affected, with a view to give relief from present pain and to facilitate motion, was the improvement to which I laid claim, assuring her, that whoever made trial of the remedy, would find their account in it. This piece of information appeared to her so very frivolous, that she never returned an answer. I met her on the street some weeks afterwards, when I inquired if she had made trial of the *bandage?* She said she had tied a bit of flannel above the knee joint, but as she had had no return of the pains since I saw her, she could say nothing about it. I plainly saw "my cure" was too simple, cheap, and of easy application, to gain the least credit with this lady. This conversation took place about the beginning of November last, and I was determined this patient should speak to me before I spoke to her again on the subject. She did so. On the 1st of December I was abruptly stopped by her on the street, when she hastily exclaimed, "It will do, for had it not been for your *bandage,* you had not met me to-day. I was seized yesterday morning with my pains, in a most violent degree, when I was glad to have recourse to the *bandage* as you directed, which gave me instant relief, and I was able to walk abroad with the utmost ease, a thing I could not otherwise possibly have done." This I considered a great triumph over prejudice; and as the patient is a sensible woman, and of unimpeachable veracity, her account may be relied on. This I hold to be an interesting case in every point of view, and quite decisive of the immense utility to be derived from the use of bandages, to those afflicted with rheumatism. That a lady for many years afflicted with rheumatism, to such a degree as to render her permanently somewhat lame of one leg, and all the year over more or less subject to pain, which in the winter season became at times so severe as to confine her, if not to her bed, at least to her room, should, from the first trial of the *bandage,* in one of these paroxysms, be not only relieved from pain, but enabled to walk abroad in cold damp weather, with a firmer step than she had done for eight years before, is a thing that speaks for itself.

On the 10th of December I again met this lady on the street, when she accosted me in these words: "I am not near so lame now." This was a piece of information that, I confess, I did not expect. I would have been quite satisfied with being told that she had experienced considerable benefit from the bandage, in the way of preventing pain. The ten days, it is material to observe, that had elapsed since she first applied the *bandage*, consisted of weather the most inconstant and severe. I therefore now put it seriously to this lady, If she was convinced that the application of the *bandage* had warded off the pains with which she was threatened on the 1st of December? If she believed the strength her leg had recovered was to be ascribed to the *bandage*? If, from what she had experienced, she believed that *bandages* would be found a powerful remedy for rheumatism? All these questions she answered most decidedly in the affirmative.

On the 15th December, William Graham, aged 32, a private in the Cumberland militia, complained of violent *lumbago*, which rendered him unable to mount guard, and he was unwilling to be sent to the hospital. On examining the part affected, found he wore a broad *bandage* round his body. This he had been accustomed to do, but said he experienced from it no benefit to his back. I asked his pocket-handkerchief, which I formed into a compress, and laid it on the part affected, applying the *bandage* over it as tight as he could bear it. On inquiring how he felt, he replied, "Perfectly at ease. I feel a want supplied, and I can turn myself any way I please with perfect freedom." He went to bed quite well, but in the morning found himself much worse. I saw him again on the 16th, about three o'clock afternoon, when I asked him if he had laid aside the *compress* that his pains had returned? He said he still wore the *compress*, and experienced from it so much benefit, that without it he could not walk at all. On the 15th I omitted to examine his pulse, which I now found at 100. I therefore immediately took twenty ounces of blood from his arm. This took off the phlogistic diathesis of the system, and the compress, which, as the pain had extended, I ordered to be enlarged, completed the cure.

In the first week of January, Mrs M. a lady about 36 years of age, complained of severe pain in the left lumbar region, for which she had been advised to have blood let. On applying to me, I recommended a trial of a *compress* and *bandage*. Having never heard of such a mode of cure, she appeared at a loss whether to take my advice as a joke or not. On assuring her I was serious, she promised compliance as soon as she got home. I insisted on the trial being made immediately; and as I had been setting a fractured arm just before she came in, there was a piece of flannel lying in the room, of which I made a *compress*, directing her to lay it on the part affected, and to make use of her handkerchief for a *bandage*. When I returned to the room I purposely introduced another subject of conversation, that what observations she might make on the mode of cure we had adopted should come spontaneously from herself. Accordingly, I soon observed her moving the trunk of her body, without having received any directions from me to that effect, backwards, forwards, and sideways. I asked her what she meant by so doing. She said the pain was gone, which yet she could scarcely believe, and that, to ascertain the fact, she was practising those motions which she dared not attempt before the *compress* and *bandage* were applied, but which she could now do with ease. Next day the pain struck her in the right lumbar region, from which also it immediately removed on application to the part of the *compress* and *bandage*.

When I relate such instances of the immediate and great good effects of *compression* in *lumbago*, candour and truth demand the acknowledgement, that I have met with several cases in which that mode of treatment completely failed of success. The superficial observer, alone, however, will from hence conclude against the utility of the practice; for the muscles of the loins are not all situated on a plane. Some of them are superficial, others deep-seated. Compression may affect the former; it cannot the latter. To deny, therefore, the utility of bandages in the cure of rheumatism, because cases occur which admit not of compression, would be as unscientific as to say, that, because blood-vessels are sometimes ruptured or wounded, which, from their situation, it is not in the power of man to coerce, Ambrose Parey, in introducing the tying of arteries in amputation, is not the author of the most important improvement ever made in the operative branch of the healing art; or, because we meet with ulcers so vitiated as to admit of being cured by no mode of treatment, that the use of bandages, as introduced by Mr Baynton for the cure of old ulcers, is not the most valuable improvement that medical surgery has received in modern times. *Cinchona* does not always cure intermittent fever, nor mercury syphilis. But would any man be so mad as to reject these remedies as not generally to be depended on, for the cure of these diseases?

On the 16th January, ————, residing at Cramond, came to consult me about a rheumatic affection, of many years' standing, in his left haunch. His knees and thighs had also been affected, and are still feeble, but free from rigidity and pain. Walks with a timorous irresolute step, the one foot often involuntarily kicking the other. I applied a bandage to both knees, beginning a little below, and carrying it a little above the joints. This gave him a command of his limbs, he said, to which he had been a stranger for many years. His principal complaint, however, was situated immediately under the crest of the left *ilium*. As soon as I ascertained this, I was sure of being able to afford him relief; for the smooth surface of the bone I knew would present an equable resistance to pressure from without. I began, therefore, with desiring the patient to perform the various motions of which the trunk of the body is capable in its sound state. But this was a task to which he was altogether unequal. His body, indeed, had been confined to a prone posture for a great length of time. He could tie the shoe of his right foot with ease, but could not reach farther than the middle of the left leg. I put him, therefore, in this position, desiring him to point, at the same time, to the spot that checked his stooping farther. This done, I pressed my hand against the part affected, and desired him to put his hand down to his foot. This he did at once, and with facility, to his own astonishment, and to that of his son in-law, who looked on. Having several times practised this, and other motions of which he was formerly incapable, I applied a compress and bandage, formed of such materials as were at hand, and sent him away rejoicing.

On the 9th of January, I was called to Mr B. a gentleman traveller, lodged in the Black-bull inn, who informed me he had been arrested on his journey, for four weeks before, by acute rheumatism, and that he had been attended by ———. His father, who had come from Whitehaven to see and carry home his son, having heard, in the town, of the successful application of bandages in the cure of rheumatism, mentioned, on his return to the inn, the circumstance to the patient, who took no rest till his father brought me to him. I stated the impropriety of my interfering with the patients of another practitioner in his absence. But the young gentleman told me, "he had not seen his doctors for some time; that he conceived they did not intend to visit him any more, he having been out; and, at any rate, he was not to be deprived, on account of any trifling piece of etiquette, of the benefit of the *bandages*, if benefit was to be obtained from them."

This gentleman I found convalescent, but complaining grievously of pain in his left hand, which was also much swelled; of pain and stiffness of the left elbow; of pain and weakness about the left shoulder; of pain and rigidity of the muscles of the back of the neck; of weakness and pain upon motion, all along the spine; and of pain and stiffness of the left knee, the outside of which he could scarcely suffer to be touched.

I began with desiring the patient to shew me how he could walk. He moved and threw his weight upon the left leg, slowly and timorously, as if afraid to trust himself upon it. But no sooner was a bandage applied, than he planted his foot firmly, and walked with great freedom, declaring that, in respect of his knee, he felt quite a different man. I next proceeded to examine the state of his left arm, and found the range of motion it possessed very circumscribed indeed. He could not even put his hand to his mouth. Upon application, however, of a *bandage* from the points of the fingers to the elbow-joint inclusive, he could do it freely. On account of the patient being very much fatigued, I did nothing at this call to the shoulder, farther than grasping it with one of my hands, in order to shew him what could be done. Upon this, he moved his arm with great facility in every direction.

January 10th.—Passed a good night, having slept from eleven to five without interruption. Pain of the left hand and knee greatly abated; in other respects as before. Removed the *bandages* and ordered the diligent use of the flesh-brush, for a considerable time, to the parts affected, which was extremely grateful to the patient. Applied the *bandages* as before, and also to the left shoulder, making a turn under the right, by which two breadths of the *roller* were made to press upon the back of the neck, a circumstance that also gave much pleasure and satisfaction.

January 11th.—Pain of the left knee, wrist, and elbow almost gone. Muscles of the neck also much relieved. Swelling of the left hand greatly abated. Motion of the left arm entire, and much more vigorous. Omit the *bandages.*

January 12th.—Has slight pain and stiffness to-day, in both ankles. In other respects improving. Swelling of the left hand entirely gone. Applied *bandages* to both ankles.

January 13th.—Was out yesterday; but says the pain and stiffness of his ankles were such, that he could not have walked at all without the *bandages.* Is resolved to have *a set of them* to carry along with him. This, I think, sufficiently indicates the opinion he entertains of their utility. It is, indeed, in such cases as this, that the pre-eminence of *bandages* in the cure of rheumatism is most conspicuous. Let us suppose that *bandages* had not been applied to this gentleman's ankles,—he must of course have remained at home; let us suppose that I had confided to rubefacients and flannel, for the removal of his pains, when would the desired effect have been produced? in a day? a week? a month? No man can answer these questions. One thing only is certain,—that the longer a patient in such circumstances is confined to the house, the longer is his general health of being restored. But no sooner were the *bandages* applied to this gentleman's ankles, than he was enabled to walk abroad. The consequence was, he returned with an appetite greatly improved, and with a universal glow and moisture of the skin, which are most successfully promoted by action in the open air, and of more real benefit to a *rheumatic* patient, than all the sudorifics in the world.

On the 15th of December last, I was called to a young man, Simon Stewart, at the west end of the town, whom I found in a condition that almost beggars description. Pale, ghastly, emaciated, he had been a martyr, for three years uninterruptedly, to rheumatism in its most cruel forms. His fore-arms, half bent, felt like two boards, and were so immoveably fixed in the prone position, that he could not see the palms of his hands till they were elevated above his eyes. Had no motion at the wrists; the right *carpus*, especially, was pained, swelled, distorted, and the command he had over his fingers was so small, that for a considerable time he was under the necessity of being fed by others. In the left knee, there was great effusion and pain; it had very little flexion and extension, being almost fixed in the half-bent posture. Both his ankles were pained, swelled, and distorted. He had not been able to stand even, on the soles of his feet, for the last twenty-three months. Sometimes, indeed, within that period, he had walked a little, but he did so by turning his heels outwards, and resting his weight on the outer edges of his feet. In this way he would crawl from his bed, along a range of chairs, to the fireside. Such was this man's situation when I first saw him. At the beginning of his illness he had declined going to the hospital, where he was offered the warm-bath free of expence; but had, notwithstanding, been attended by a host of medical men in succession: one of these, in particular, had done every thing for him that skill could devise, or philanthropy suggest, but, long before I was called in, had also given him over as incurable. Having satisfied myself as to the state of the patient, I applied a roller of flannel to the arms, from the wrists to the elbow-joints inclusive; in like manner to the knee; and, lastly, to both ankles. This done, I desired the patient to come out of his bed and dance! He descended very slowly and timorously, but getting his feet to the ground, and finding he could stand, he at last stepped out, declaring, that if his knee had not been bound so tightly he could have leaped the height of his stick.

December 21st.—Has had great pain in the knee, especially in the night-time, ever since the *bandage* was applied, which has therefore been laid aside. Some rotatory motion of the fore-arms. Apply a compress of flannel to the *aponeurosis plantaris* of the left foot.

December 25th.—Rotatory motion of the fore-arms increasing; motion of the left wrist very considerable; had none at the beginning. Some pain among the *tarsal* bones of the left foot, but can stand on both soles. The *compress* to the left foot a great improvement to his walking, to which, he thinks, there is no obstacle but the knee.

December 28th.—Fore-arms more plump in their appearance, and the skin of a more kindly feel. Walked from his bed to the fire-side, without a stick, or any other support. This is the first time he has done so under my care. Feet quite the natural shape, and sets them down without fear or dread. No impediment to his walking abroad but the knee. Can turn his hands half-way to the supine posture.

℞ Muriat. ammoniæ, ℥i.
Acid. acetos. commun. ℥xii. Solve.

To be rubbed with great diligence all over the knee-joint, twice a-day.

December 31st.—Circumference of the knee diminished three-eighths of an inch. Attempted to walk without the bandages about his feet and ankles; could not set down his feet with confidence and firmness. Applied the bandages, when he walked with the utmost ease, knee excepted.

January 5, 1815.—Had pain among the tarsal bones of the right foot night before last;—gone to-day. Thermometer yesterday and to-day at 40; weather hazy and damp. What is the reason that people subject to rheumatism are so much affected upon the change of weather? I firmly believe it is owing to sympathy between the parts affected and the lungs. This is no extravagant idea. According to our theory, the white parts are the chief seat of rheumatic affections. Now, a great proportion of the lungs consists of cellular substance. Besides, we every day observe sympathies much less explicable than this: between the parotid gland, for instance, and the testes; the kidneys and the stomach; and, what is more directly in point, betwixt the urethra and eye, in gonorrhœa. I am aware of a question that may be started here, namely, Can a part distant from the lungs sympathize with them, or be affected through their medium, without their being themselves sensibly affected? I would answer this question in the affirmative. I think it very rational to suppose, that a part already habituated to a particular disease, a disease too that gains ground by repetition, may become severely affected by sympathy with another part that has been but slightly or imperceptibly affected. In no other way can we explain the fact, that persons subject to rheumatism can, in the morning, before they get out of bed, or even in the nighttime, prognosticate with certainty a change of weather. In such cases, the effects produced in the system, by the change that takes place in the atmosphere, must be through the lungs only, for no other part of the body is exposed to its action.

Knee has not been at all troublesome for some nights past. Took off his stockings last night with his own hands, a thing he has not been able to do for twelve months past. Is gaining flesh. The fore-arms, it is remarked by strangers, are more plump than formerly.

January 6th.—Had severe pain last night in both ankles, but especially in the right, and also in a slight degree in the elbow-joints; motion of these parts, however, no way impaired to-day. On inquiry, I found he sat an hour yesterday after my visit, with his clothes hung round him, for want of aid to put them on. Has some catarrhal symptoms, with slight cough. Here I may remark, that I have uniformly observed the return of pain, in this patient, to be accompanied with more or less of catarrhal symptoms,--an observation corroborative of what is advanced in yesterday's report, and indeed of our theory, and the whole reasoning on it, from beginning to end.

℞ Vin. ipecacuan. ℥i.
Tinct. opii, gtt. xl.
Aq. font. ℥vi. M.

the half to be taken an hour before, the other half at bed-time.

January 9th.—Has got cold anew. Coughed a good deal last night; severe pain in the ankles, wrists, elbows, and left-knee. Cannot walk to-day, nor suffer the bandages so tight as usual. Keen frost yesterday; thermometer at 40 to-day; weather boisterous and damp.

12th.—Is able to walk again, but has still some pain among the tarsal bones of the right foot. I do not expect this patient to make any farther progress during the winter months, and shall be very glad if he retains what he has acquired. The bandages, it must be admitted, have done wonders; but they are not a match for an inveterate disease, weather variable in the extreme, an open house, with the patient's bed at the back of the door, a clay floor, and imprudent conduct, combined. I therefore close this case for the present, trusting I shall be able to give a more decisively favourable account of it hereafter.

From the preceding details it will perhaps be inferred, that it is in *chronic* rheumatism chiefly, that decisive effects are to be expected from *bandages.* This may lessen, in the estimation of some, the value of the remedy. But if it is attended to, that *chronic* is as often a cause as a consequence of *acute* rheumatism, that *chronic* rheumatism occurs out of all proportion more frequently than *acute*, *bandages*, as a remedy applicable with the fairest prospect of success in so many instances, cannot, if viewed impartially, be considered in any other light, than as the accomplishment of what has hitherto been a *desideratum* in the cure of rheumatism. To render this evident, we have only to reflect how often every mode of cure of that formidable disease proves abortive. It certainly must be ascribed, as well to the imperfection of the healing art, as to the obstinate nature of the disorder itself, that so many, in every rank, are subjected, during a great part of their lives, to the visitations of this horrible malady. *Bandages* bid fair to supply this deficiency. They are not like medicines exhibited internally, concerning whose operations and effects we are liable to form the most erroneous conclusions. Their effects are immediate and visible. We are not left at a loss to determine, whether the good produced is to be ascribed to the efforts of nature, or to the remedy employed; nor is it a matter of doubt, whether the remedy is of real advantage or not. There can be no uncertainty as to the powers of a remedy, which, the moment it is applied, enables a person to walk, who immediately, and for many weeks before, could not set the sole of his foot to the ground.

But were it even admitted that *bandages* are applicable, with effect, principally in *chronic*, it is evident that they may operate, in many instances, as preventives, at least, of *acute* rheumatism. This is a fair inference from the girl's case above detailed. In her were present, though the symptoms did not run high, all the characteristics of *acute* rheumatism. But, by the timeous application of *bandages*, not only were the pains immediately relieved, but the fever soon after subsided. A most useful practical hint this—shewing that, as the derangement of the system was occasioned by local affection, so the melioration of local symptoms had its influence on those of the system. Here naturally arises the question, May not *bandages* supersede the necessity of repeated detractions of blood in *acute* rheumatism, even when local affections are not the primary symptoms? At any rate, *acute* rheumatism is soon subdued by the lancet; and, if *chronic* rheumatism supervenes, the application of *bandages* in this stage, will, we have reason to believe, prevent that lingering illness, general debility, rigidity and coldness of the limbs, pain and stiffness of the joints, so often the *sequelæ* of *acute* rheumatism.

Every practitioner, surely, of even very limited experience, must have met with cases of *acute* rheumatism, in which the difficulty was, not to subdue fever, but to set the patient on his legs, and, having set him on his legs, to make him walk. In this stage of such cases, more benefit, I hesitate not to pronounce, will be derived from *bandages*, than from all other topical applications that have ever been prescribed by the regular physician, or advertised by the empiric.

But a mechanical remedy that possesses, in any degree, the power of moderating or preventing *acute*, must operate with tenfold effect in cutting short *chronic* rheumatism. Every body knows that, whoever have once become acquainted with rheumatism, may lay their account with a repetition of its visits; and the oftener it comes, and the longer it stays, it strikes its roots the deeper:—this to such a degree, as, in many instances, to render the patients miserable during its stay, and uncomfortable all the rest of their lives. The early application of bandages, by promoting, in the way formerly stated, circulation in the capillaries of the white parts, prevents their permanent obstruction, and consequently the disease from taking such hold of the system as to render it difficult afterwards to be eradicated. That *bandages*, when early applied, must produce such effects, is proved by what they have done in Stewart's case; for, if they possess such powers in old inveterate instances of the disease, they must, *a fortiori*, be productive of like benefit in recent cases.

Summary of observations made on the effects of *bandages* in rheumatism.

At certain times, in some cases, but especially at the beginning, it is necessary to make the bandages tighter than they can be well borne for any length of time. In such cases, frequent removal of the bandages, and friction in the intervals, are indispensable.

The pains and rigidity of parts do not return immediately on the removal of the bandages; the effects remain for a considerable time after the cause is removed.

Parts that have for a considerable time been treated with bandages come at last to be covered, when the bandages are at any time removed, with a copious, warm, and fluid sweat, which gives a kindliness and pliancy to the skin, which it did not possess before.

Parts that have for a considerable time been treated with bandages acquire plumpness and strength, while parts of the same person, that do not admit of being bandaged, remain emaciated and tremulous as before. This observation is demonstrated in Stewart's case.

IV.

Two Cases of Dislocation of the Thumb. By George Ballingall, Esq. Assistant Surgeon 1st Foot, or Royal Scots. Communicated by John Barclay, M. D. Fellow of the Royal College of Physicians, and Lecturer on Anatomy, Edinburgh.

About Christmas 1812, private Peter Macphail, of the Royal Scots, a stout, healthy young man, had his right thumb dislocated in the manner mentioned by Mr Hey of Leeds. The proximal extremity of the proximal phalanx was thrown upon the anconal aspect of the metacarpal bone, * while the distal extremity of the latter projected in towards the palm of the hand. In this position the bones were found about fourteen hours after the accident. In the interim, some attempts had been made by his comrades to reduce the luxation, and the parts were in consequence a good deal swelled. Every effort made by the surgeons of the regiment to effect the reduction proved unsuccessful, and leeches being applied round the joint, we desisted from any farther attempt until the swelling should subside. Mr Johnson and I, in the mean time, dissected this joint in an emaciated subject, who died of dysentery, and, previous to opening the joint, endeavoured to dislocate it in the manner above mentioned. This, however, we could not effect until the surrounding teguments were removed, and the flexor and extensor tendons passing over the joint divided. When the bones were brought into the wished-for position, we thought an impediment to the reduction was perceptible in the action of the abductor and adductor pollicis, to which our attention was particularly directed, as being powerful muscles, peculiar to this articulation; and conceiving the difficulty to exist in this more than in the lateral ligaments, as mentioned by Mr Hey, we agreed to bleed Macphail *ad deliquium*, and renew our efforts. This was accordingly attempted on the following morning. About four or five days after the accident, a vein being opened in each arm, syncope was very soon produced; and while the patient was in this state, a powerful extension was made by means of a lac passed round the distal extremity of the dislocated phalanx; the patient was retained in a sitting posture, and the fore-arm kept in a horizontal direction. A counter extension was also made by means of a folded cloth passed diagonally across the ball of the thumb. All our exertions to reduce the joint, however, still proved ineffectual, and the bones remain so far overlapped as to shorten the thumb very considerably, and to render him incapable of some of the necessary motions with the firelock, particularly that of cocking, in consequence of which, he is recommended to be discharged.

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* I use the term anconal here, to express the flat surface of the bone, corresponding to the anconal aspect of the other bones of the metacarpus. This, in the usual position of the thumb, would perhaps more properly be termed its radial aspect.

On the 28th of February 1813, Peter Morison, a private in the Royal Scots, had his right thumb dislocated in the same manner as Macphail. Assistance was, however, applied for in this instance a few minutes after the accident. A powerful extension was made in every possible direction, and a rotatory motion (on its own axis) was given to the dislocated phalanx. The extension seemed nearly sufficient to admit of reduction; the extremities of the bones appeared almost disengaged, but could not be made to resume their natural position; even when the extension was discontinued, the bones did not recoil or overlap to any great extent; and although the dislocation was here equally complete as in Macphail's case, the thumb was not shortened in the same degree. The patient was extremely faint, not only from the pain we had given him in making the extension, but also from the collapse succeeding to excessive intoxication; and while in this state, I was pressing gently on the anconal aspect of the joint, endeavouring to push the dislocated bone into its place, when I unexpectedly felt the phalanx slip from under my fingers, and resume its proper situation. The patient was at this time seated in a chair, the fore-arm bent to a right angle with the humerus, and the hand in a middle state between pronation and supination, while the joints of the wrist and thumb were left as much as possible free from constraint.

In Richerand's edition of Boyer's Lectures on the Bones, we have the following observations on this dislocation:

"When a violent effort is made on the thumb from before, backwards, its first phalanx slips behind the head of the first metacarpal bone, and remains extended, while the second is bent, its flexor muscle being thrown into action by the irritation. The distortion of the thumb,—the impossibility of bending the first phalanx,—and the pain,—render this luxation sufficiently evident." "These luxations should be quickly removed; for, at the end of eight or ten days, they are irreducible. Desault, in a case of this kind, proposed making an incision behind the superior extremity of the phalanx, and, by means of it, to introduce a spatula, in order to push the phalanx into its place; but the patient, frightened at the operation, would not submit to it. Citizen Boyer has also observed, in a hair-dresser, a luxation backwards irreducible, from having continued too long." "The reduction is not as easy as one might imagine. The number and force of the muscles which are inserted into the first phalanx of the thumb, and the little hold we can take of this part, in order to make extension, render the reduction difficult."

Mr Hey, again, the latest practical writer who has particularly noticed this dislocation, makes the following observations on the subject:

"A peculiar difficulty attends the reduction, when the head of the metacarpal bone, which is joined to the first phalanx of the thumb, is luxated completely, and depressed towards the palm of the hand. A dislocation in the opposite direction is easily reduced." "A transverse section of the anterior extremity of the metacarpal bone exhibits the form of a wedge, the narrowest part being towards the palm of the hand. There are two tubercles on each side of the anterior extremity of the metacarpal bone, whence the lateral ligaments go off, in part, to the first phalanx of the thumb. Upon measuring the distance of these tubercles from each other, I have found those two tubercles which are nearest the palm of the hand to be only 3-8ths of an inch from each other, when the tubercles on the posterior part of the same bone were at the distance of 5-8ths of an inch. Supposing, therefore, the head of the metacarpal bone to be pressed forcibly between the lateral ligaments towards the palm of the hand, the extremity of the metacarpal bone passes like a wedge between the lateral ligaments, and, having passed through between them, it cannot return, as the posterior broad part of the bone presents itself to the more contracted aperture between the ligaments. From an anatomical consideration of the structure of this joint, it seems impossible that the metacarpal bone can pass in this direction to a complete dislocation, without tearing off some part of the lateral ligaments; yet so much of the ligaments may remain, as to prevent the return of the bone to its natural situation." "Whether these observations account for the difficulty of reduction in this species of dislocation or not, I know, from experience, that the reduction is in some cases extremely difficult, if not impracticable." Mr Hey then goes on to mention four cases, in no one of which does it appear that the reduction was effected. Besides the cases above alluded to, another has been mentioned to me by my friend, Mr Annesley, surgeon of the Madras European regiment, in which the reduction was not effected; but, as this case did not, in the first instance, fall under Mr Annesley's immediate observation, he was unable to furnish me with the particulars. These cases, in addition to the two I have detailed, make nine instances of this luxation, in only one of which has the reduction been accomplished. It is not without much hesitation that I take upon me to question any position of an author like Mr Hey, whose opinions are entitled to so much respect; yet, considering that he does not speak upon this subject with that decision which his experience on other occasions leads him to adopt, but is simply offering an explanation of a difficulty frequently insurmountable, I hope I may be permitted to hazard a few remarks on what he has said of the impediments to the reduction of this dislocation.

Having been always taught to look for the chief obstacles to the reduction of every luxation in the action of the muscles surrounding the joint, I felt but little inclined to be satisfied with an explanation which ascribes the difficulty, in this case, to the resistance of the lateral ligaments. There are also two other considerations, which, in my opinion, irresistibly militate against Mr Hey's supposition. In the first place, it does not seem to have been sufficiently considered, that the lateral ligaments, being inserted into the dislocated bone,* will of course change their relative situation with that bone; and it is difficult to conceive how the head of the metacarpal bone can get so far through between these ligaments, as to form so serious an impediment to the reduction. In the next place, it is scarcely possible to believe that an obstacle of this nature could have so long resisted the combined efforts of three men, and afterwards be overcome by the moderate degree of force I employed to effect the reduction in Morison's case. Although I have thus stated what appear to me strong objections to Mr Hey's explanation, I feel much at a loss to offer any other more satisfactory.

Quer.—What situation does the tendon of the flexor brevis occupy in this dislocation? Is it possible that the head of the metacarpal bone may be forced through between the two portions into which the flexor brevis naturally divides, to form a groove for the tendon of the flexor longus? It is indeed true, that the two portions of the flexor brevis are described by Winslow and others, as uniting to form one tendon, implanted into the proximal extremity of the proximal phalanx. Still, I think it possible that the fibres of these two portions may remain so far distinct, as to admit of the head of the metacarpal bone being forced through between them, and if so, the two portions of the muscles, when thrown into action, would grasp the metacarpal bone, and present an obstacle to the reduction, precisely in the same way that Mr Hey supposes the lateral ligaments to do, but much more considerable, in as far as the resistance of the living muscular fibre exceeds that of ligament. When the dislocation has been artificially produced in the dead subject, the abductor and adductor muscles of the thumb, will be seen passing obliquely across the metacarpal bone, from the thenal to the anconal aspect, and, when thrown into action, will present an additional obstacle to the reduction, by tending to lock or bind the bones together as it were, and to retain them in the position they now occupy. If these are the true impediments to the reduction of this luxation, the impropriety of making the counter-extension, as was attempted in Macphail's case, will be obvious, because a lac passed across the ball of the thumb, will compress the bellies of the muscles concerned, and excite them to action, so as to increase the resistance. Whatever may be the nature of the obstacles to the reduction of this luxation, the difficulty of the case is sufficiently established. Every consideration induces us to suppose, that some extreme violence is done to the soft parts, of which the nature can only be elucidated by dissection; and should any of your readers have an opportunity of throwing light on the subject, by dissecting the joint in its dislocated state, it may prove highly satisfactory, by leading to a more successful practice in future. The accident is, I believe, a rare one; but still it is one we would all wish to be able to meet with confidence. My success in Morison's case, is in a great measure to be attributed to the early period at which the reduction was undertaken; and it appeared, after all, so much the result of accident, that I do not feel myself competent to lay down rules for effecting the reduction on any future occasion. And although I have no wish to attach any undue importance to these cases, I cannot help reflecting upon the loss of confidence, and other disagreeable consequences that might attend a failure in a case of this kind, were it to happen to a young surgeon commencing his career of practice in private life; while to the military surgeon, it cannot but be highly distressing to lose the services of a gallant young soldier, from an accident apparently so trifling.

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* It is to be observed, that the lateral ligaments are merely portions of the capsular ligament, strengthened by additional fibres, at those points where it is unsupported by muscles or tendons passing over the joint.

Bangalore, March 8th 1814.

V.

Description of a substitute for Leeches. By JOHN WELSH, Surgeon, Haddington.

IN a late pamphlet on cupping, by Mr Mapleson of London, it is affirmed, that the adroit performance of the operation continues to be confined to a few individuals in that capital. I never witnessed the operation by any of these gentlemen, but it has been minutely described to me by a person who had undergone the operation himself, and seen it repeatedly practised upon others. The neatness and expedition with which they abstract even large quantities of blood fully justify the frequent recourse which is had to their assistance in London practice; and notwithstanding the great number of scarifications inflicted by three or four applications of a scarificator, containing sixteen lancets, the pain, I am assured, is so inconsiderable, that patients readily submit to the operation. Mr Mapleson's professed object is to instruct surgeons in the most approved method of cupping; but it may be doubted, whether any directions, however explicit, or even the occasional performance of the operation with the apparatus now in use, can enable surgeons in general to rival in any degree the dexterity of those who practise this operation exclusively.

I have been long in the practice of procuring a discharge of blood, both from scarifications and the bites of leeches, by means of cupping-glasses, originally intended to be used with an air syringe, employing merely suction with the mouth; and I can with much confidence recommend this, probably the most ancient mode of cupping, as the simplest, the most certainly effectual in the hands of those not much accustomed to the operation, not liable to the objections which apply to the air-syringe, and capable of wholly superseding the spirit of wine-lamp, in the use of which alone the dexterity of the professional cupper is admitted to consist.

As the brass cape and screw with which the cupping-glass used with the air-syringe is mounted, render it rather inconvenient for suction by the mouth, I have lately had some formed entirely of glass, of the ordinary sizes and shapes, except that they have a cylindrical mouth-piece, about an inch in length, projecting from their apex. On the superior aperture of the mouth-piece, a valve is formed, and at any time very easily renewed, by tying on a small bit of smooth bladder. It might perhaps add to the neatness of the operation, to have the tubular part of the glass of much greater length, by enabling the operator to keep his head farther removed from the patient. But this, in some situations, might prove inconvenient, and would render the glasses less portable, and more liable to accident. These cupping-glasses may be had from Messrs Boogs, cutlers, Edinburgh, with whom one is left as a pattern; and any gentleman, without proceeding to the operation of cupping, may satisfy himself of their simplicity and effect, by merely dipping the edge of the glass in water, applying it to the palm of the hand, and employing a little suction; or, by sucking the air from it, inverted upon water, when it will be found that it may be entirely exhausted by two moderate inspirations; and that the valve completely excludes the air upon the suction being discontinued. This facility of exhaustion suggests the necessity of caution, not to carry it too far in cupping, which, as M. Mapleson observes, impedes the flow of blood, by compressing the cutaneous vessels. Before applying this cupping-glass, the valve should always be moistened with water, and ascertained to be pervious.

The scarcity of leeches for some years past, appears to me to impose it as a duty upon surgeons to become more familiar with the practice of cupping, more especially among the lower orders, who must otherwise, from not being able to procure leeches, often forego the advantages to be derived from local blood-letting. When, from the timidity of patients, or other reason, leeches are preferred, instead of the tedious and often ineffectual fomentation of the bites, a sufficient quantity of blood may, in general, be obtained in a very short time with these glasses, particularly if the leeches have been applied in groupes, so that each glass may include four or five bites.

Haddington, January 14, 1815.

VI.

Case of extensive Wound of the Throat. By Robert Hathorn, Surgeon, Donaghadee, County of Down, Ireland.

On the 14th of March 1814, I was, in great haste, called to visit J. M'R. æt. 58, a respectable farmer, about six miles distant, who, I was informed, had, in a fit of despair, inflicted two desperate wounds on himself, in consequence of which he was expected every minute to expire.

I repaired to his house with as much expedition as possible, and on my arrival found him in the following frightful situation:

His throat was cut from one angle of the lower jaw to the other; the trachea was completely separated from the larynx, and the two cut ends had retracted to a very considerable distance, leaving the œsophagus exposed to view. Respiration being carried on *entirely* through the wound, suffocation had very nearly taken place, from a large coagulum of blood being situated over the orifice of the trachea. After securing by ligature three small arteries that were springing, and having removed the coagulated blood, and, consequently, the danger of immediate suffocation, I turned my attention to a large wound in the abdomen. It extended transversely from one side to the other, about half an inch above the navel, and was about nine inches in length, of which nearly four inches in the middle had penetrated the cavity of the belly, and through which portions of the intestines and omentum had protruded. The tumour formed by these was about the size of an infant's head. The omentum was cut in several places, and blood oozing from its surface. On the whole, the hemorrhage from this wound, considering its extent, was but trifling. I could not possibly ascertain whether the epigastric arteries were wounded or not, but, from the result of the case, am inclined to believe they had escaped the ravages of the maniac's knife.

After placing my patient in a proper posture, by relaxing as much as possible the abdominal muscles, I endeavoured to return the intestines, and next the omentum, into their natural situation. That this might be the more effectually done, it became necessary for me to introduce two of my fingers within the abdomen, and push the omentum down towards the pubes, and past the inside of the wound, to prevent a second protrusion, and consequent strangulation there. Having, with very considerable difficulty to myself, and, as may well be conceived, pain to my patient, succeeded in this, I brought the edges of the wound accurately into contact, and retained them so by eight stitches, slips of adhesive plaster, a pledget of lint, and, to secure all, a circular bandage, applied with a moderate degree of tightness. I then returned to the wound in the throat. After bringing the orifices of the trachea and larynx together, and retaining them in this situation, I closed the external wound in the same way as that in the abdomen. I brought the chin into contact with the breast, by raising the head with pillows, and gave strict orders to have it preserved steadily in this posture. Respiration went on now uninterruptedly, and my patient could even speak to me in a low tone of voice, and say that he felt very easy.

I visited him again on the 15th, and found that he had had a tolerably good night. His pulse was 90, and full; had but trifling pain in his belly, and little uneasiness from the throat. With the intention of *keeping off* abdominal inflammation, which I had so much reason to dread, I took 16 ounces of blood from his arm; and, as his bowels were costive, gave him a dose of castor-oil. Allowed him no food but a little panada, and for drink, toast and water.

On the 16th, the castor-oil had operated well. Passed a restless night; complained of smart pain all over the abdomen; had vomited some in the morning, and was still sick at stomach. Pulse 116, and sharp, skin hot and dry, tongue furred, thirst great. I again took 12 ounces of blood from his arm, ordered him a saline diaphoretic mixture, and the free use of oranges.

On the 17th, the pain in the abdomen was a little abated in the night, but had returned towards morning with double force. He had repeated vomiting, and was teazed a little with a tickling cough. The pulse was 128 and sharp, skin a little moist, tongue dry, thirst continued.

For the first time, I opened the wounds, and began with that of the abdomen. On removing the dressings, to my great surprise and mortification, I observed, on the centre of the wound, down to the navel, a livid red tumour, about the size of a large pigeon's egg, which, on examination, proved to be a portion of the omentum that had protruded between two of the stitches, had swelled to the above size, and was affected with gangrenous inflammation.

Conceiving the reduction of this tumour impracticable, without a dilation of the wound, (for by this time the adhesive inflammation had made considerable progress in closing it), and being of opinion that the presence of such a tumour in the abdomen might, from the degree of inflammation with which I had every reason to believe the bowels were affected, have been very hazardous, I resolved on removing it by the knife.

Having done so, I applied compression with a soft sponge, to the small cut vessels of that part of the omentum which remained in the wound, till the bleeding had stopt. I then, with one finger, endeavoured to push this into its place.

Having done so, and withdrawn my finger, I put a deep stitch into the aperture through which it had protruded, and dressed the wound as before. I opened the wound in the throat: it looked very well, and was dressed as the other. I took 12 ounces of blood from him again, and ordered him a dose of castor-oil. The saline mixture, &c. were continued.

On the 18th, he had a tolerably good night, the pain in the abdomen being much abated. Had no returns of vomiting, but some sickness from the oil, which operated well. The pulse was 106, and soft; skin moist; thirst diminished a little; tongue white and soft. The wounds looked well, and were covered with pus. Cough was more frequent and teazing, particularly in the night. For this I ordered a mucilaginous mixture, with opium, and the saline mixture was continued.

On the 19th, the pain in the belly but trifling, and chiefly confined to the region of the wound; no sickness at stomach. Pulse 94, and soft; skin moist; tongue cleaning; thirst moderate; wounds looked well, and dressed as usual; the cough easier. The medicines were continued.

On the 20th, was much the same as on the day before. Medicines were continued.

On the 21st, the pulse was 90, and soft; had no pain in his belly, except a little griping by turns; skin, thirst, and tongue, nearly natural; cough easier; wounds healing fast. Medicines were continued.

On the 22d, was quite free from pain in the abdomen. Pulse 80, and soft; other functions the same as the preceding day; wounds doing well. His bowels being rather tardy, I gave him some castor-oil. The saline mixture was stopt, but the cough-mixture continued.

On the 23d, the castor-oil had procured two healthy stools; was quite free from pain. Pulse, and other functions, natural. Took some panada with appetite. The wounds looked well, and were dressed as usual. Cough still a little teazing in the night; the mixture continued.

On the 24th, the febrile symptoms were quite gone; appetite improved; wounds healing fast; cough easier. Mixture continued.

On the 25th, cough was nearly gone; he felt very easy and comfortable; wounds doing well. Mixture stopt.

On the 26th, all was going on well. In four weeks from the receipt of the wounds, they were perfectly cicatrized, and in every respect he was in very good bodily health.

His mental disease, however, I am sorry to say, still continues.

Donaghadee, County of Down, Ireland.

VII.

Case of Tetanus cured by Injections of Tobacco Smoke. By THOMAS DUNCAN, Surgeon, Grenada.

THE following case of tetanus is communicated, not with the view of exalting the merits of a plan, which, in this solitary instance, has succeeded, or of raising my reputation on the result, but of drawing the attention of my medical brethren to the management of a most formidable and fatal disease, in which the same method of treatment, in two successive occurrences, cannot be depended upon, and of inducing a trial of remedies not in general use, although I think, in some degree, warranted by the appearance of the disease. The tonic and stimulating plan of bark, wine, mercury, and cold bath, has had the suffrages of Drs Cochrane, Wright, Rush, Currie, Hosack, and others, and, no doubt, deservedly; but I think the latter goes too far when he says, that the failure of these must be attributed to the complicated and inert manner of giving them, and not to the principle of their action; because the proximate cause of the disease is an exhausted state of the sensorial power, from violent irritation, applied to the nervous system. (Vide Duncan's Annals for 1799.) Although I cannot enter upon the nature of a disease so little understood, experience authorizes me to say, that neither antispasmodics nor tonics, sedatives nor stimulants, either by themselves or variously combined, can be relied upon, even under the most faithful administration, for cures and failures have taken place under each. It is no wonder that Boerhaave says, "Ex his vanitas apparet omnium specificorum et methodorum quæ inanis jactantia contra hoc malum laudat;" and that, meeting with continual disappointment, we fly to every thing that gives but a ray of hope, even out of the routine of common practice.

Sylvest, the subject of the present communication, is a negro attached to Palmiste estate, the property of Bogle French, Esq. of London, a stout and hale young man, of 20 or 21 years. On hoisting up a mill-roller, the tackle gave way, and the edge of the iron cylinder fell on his left foot, which produced excruciating pain. When called, I found considerable laceration and contusion of the metatarsus, without, however, exposing the bones, or, as far as I could discover, fracturing any of them. As is usual in such accidents, there was little hæmorrhage. The wound was dressed with spt. terebinth. which Dr Rush, in the Medical Inquiries, gives us reason to suppose is a perfect prophylactic against tetanus; and at night an anodyne draught was ordered. During the first three or four days, warm poultices and fomentations were employed to produce suppuration, and the draughts continued. A slight sphacelus followed, which being removed on the 9th day, left the sore clean, and discharging good pus. Until the 15th, matters went on favourably under the opiate at night and emollient dressings; the pus was healthy, and the granulations advancing; but on that day he complained of stiffness in the muscles of the neck, and difficult deglutition, which being taken for cynanche tonsillaris, then very prevalent, owing to the spring rains, was unnoticed. At my visit the following morning, viz. the 18th May 1812, the circumstance was accidentally told me, which, from repeated experience of this simple and insidious commencement, called my immediate attention. On desiring him to show his tongue,—a question I prefer to any other, as it conceals suspicion,—I found he could not separate his teeth sufficiently to put it out. I foresaw what was approaching, and having given him tinct. opii gutt. lx. in a glass of mulled wine, and directed the neck and fauces to be well rubbed with the former, I sent for my brother, with whom I had laid down the treatment of the next case of this kind which should occur, having lost the two preceding by the tonic and stimulating plan; but before he came, I was called to some distance off, and did not see Sylvest again until next morning. In the mean time, my brother ordered the warm bath, gutt. lx. tinct. opii every third hour, in a glass of mulled wine, unless sleep should be produced, and a warm poultice sprinkled with pulv. opii to the sore.

Next day, the 19th, we found all the symptoms of a confirmed tetanus present—the jaws locked to within one-eighth of an inch—violent convulsions and contorsions backwards of the whole body occurring every few minutes, and increased by attempting to drink, or when spoken to—that pathognomonic sign, the pain under the sternum, excessive, and, as he expressed, unbearable;—slept none, although he had taken the laudanum three times during the night, and once this morning. Pulse 88, and firm; skin cold, and covered with perspiration; belly costive; sore looking well.

> Adeat baln. tepid. pro hor. dimid. bis indie; postea bene fric. ol. olivar. calid. præcipue spina dorsi, et exhib. enema fum. nicotian. toties.
>
> Cap. tinct. opii ʒi. in vin. Mad. ℥ii. omne nocte.

We saw him in the afternoon, about five hours after, using the bath, &c. He was in a profuse perspiration, being covered up with blankets. The enema produced fainting, which lasted some time. On recovering his senses, he expressed himself well, and unconscious of being sick; had a large hard and fetid stool soon after. No abatement, however, of the symptoms, although the spasms did not recur for half an hour after the syncope. Wishes for heat. Let him be wrapped in flannel, well covered with blankets, and have a charcoal fire burning close to him night and day.

20th.—The enema yesterday afternoon produced only nausea and sickness. Sweated much, and continues to do so. The jaw is so firmly fixed, that the thinnest fluids with the utmost difficulty pass, and he is averse to take any, from the severe spasms which are consequent upon the action of swallowing. Opisthotonos unabated; the fits return every few minutes with increased anguish; feels the charcoal fire pleasant, and objects to any detraction from its heat, which is so great as to be uncomfortable to a bystander. Sore looking kindly, and yielding a moderate quantity of good pus; but the foot is much swelled. No sleep from the draught, nor has he closed his eyes for the last 48 hours. Pulse 94; skin warm; no stool from the injection yesterday evening. Let thin panada, with Madeira wine and strong sangrée, be sucked down frequently.

Rep. baln. et frict. ol. calid. ut heri præscript. et inj. fum. nicotian. donec superveniat syncope; necnon rep. tinct. opii hora somni.

21st.—Mouth clenched most firmly, and the spasms equally frequent, though not so severe; thinks the pain at the scrobiculus cordis less; has taken a good deal of sangrée, but can receive nothing more substantial. Sweats profusely. Each enema, after a long exhibition, produced slight syncope: felt them comfortable in his bowels, and had an operation from them. Attributes the sickness and fainting to the fumes reaching his head. Slept about twenty minutes after taking the draught, but the spasms returned with increased violence, and he passed a very bad night. Sore healing under emollient dressings; pulse 90; thinks the heat does him much good.

Cont. omnia ut heri præcript.

22d.—The enema yesterday produced complete syncope, and to-day he feels much easier. Can open his mouth a little in the absence of spasms, which, although they return as often as before, are by no means so violent. The pain at the scrobiculus cordis is diminished, and he had two operations after the injections. The sensation of fainting is so dreadful, that he says he would rather bear the convulsions, painful as they are. Feels his neck and jaws very stiff and sore; sweats most profusely, more especially after fainting. Has taken a little panada, but convulsions are sure to follow deglutition. Ulcer mending; pulse 90 and full; skin warm; slept a little.

(Considering this the 4th day of the disease, I augur favourably of its termination, for in almost all other cases I have found that one critical. The great Hippocrates in his fifth Aphorism, says, what meets my perfect concurrence, "Quos tetani vis magna exercet intra quartum diem intereunt, quodsi hos effugerunt tetano levantur et sani evadunt.")

Pergat ut heri.

23d.—An evident alteration for the better, and an abatement of all the symptoms; can open his mouth half an inch, and the spasms are neither so frequent nor severe. The draught procured some sleep; slight syncope from the injections. Sweats very profusely, to which he attributes his meliorated state. Objects so strongly to the bath that it was omitted yesterday. Sore healing, and swelling gone. Pulse 87; belly open.

Contr. alia.

24th.—Mending; has had only six attacks since last report, and can open his mouth somewhat more. The enemas are not so long exhibited as formerly, and nausea and sickness only are produced. Belly open; slept well; sweating continues; fire and blankets still used.

Habt. frict. et enem. semel indies.

Capt. cort. Peruv. ʒi. ter indies et rep. haust. anod.

27th.—Continues to improve in every respect; opens his mouth full an inch, and has from two to four spasms only per day, and these not severe. Deadly sick from the enema, which he begs may be left off. Continues the fire and warm covering, and is in continual perspiration. Pulse 90; tongue clean; belly costive.

Omitt. inject. fum. nicotian.

Cont. frict. ol. calid. Cont. etiam cort. Per. sed admisc. quotidie p. jalap. ʒss. prima in dosi.

31st.—Doing well. Opens his mouth freely, and is rarely seized with spasms. Continues the friction, sweating process, and bark; takes a good deal of nourishment, and sleeps well at night. Pulse 76; belly open; tongue clean; sore closing.

4th June.—Gaining ground daily. The heat of the charcoal fire has been gradually diminished, and is now entirely withdrawn. He is directed, however, to continue the flannel, and not to move from his bed.

Cont. cort. Peruv. et tinct. opii u. a.

11th.—Improving. No more convulsions, but great stiffness and stricture of the jaws. Is gaining strength. Got out of bed to-day for the first time, well wrapped in flannel, which he must use for some time. Functions natural, and sore healed.

Cont. cort. Peruv. sed dimin. dos. tinct. opii ad ʒss.

By the end of June, this patient was perfectly recovered from the disease, the only remains of which was stiffness and rigidity. He wore the flannel for a long time after, and is at this moment in good health, except an occasional pain and swelling of the feet in wet weather.

At this distance from the emporium of medicine and seat of improvement, it might justly be reckoned presumption in me to enter upon the pathology of a disease which has puzzled both ancients and moderns, and towards a systematic treatment of which they have done so little; but it may be expected that I should account for my practice in the case of Sylvest, unsupported as it is in some respects by great living authority, and in one entirely new.

As I have already observed, that the two preceding instances of tetanus died under a strict observance of the tonic and stimulating plan, where the cold-bath, opium, wine, and mercury, were faithfully administered, I was determined to depart from the usual routine of practice, and try a relaxant one, without, however, the aid of venesection, which, if carried to any extent, might weaken the system beyond recovery. For this purpose, I had recourse to the hot-bath, and directed warm olive-oil to be well rubbed over the whole body, more especially on the spine. The use of oil is as old as Hippocrates, who says, in treating of this disease, "Totum corpus oleo immergatur tepenti maximaque cervix hapsis foveatur." The Spanish practitioners, in the new hemisphere, use it. Dr Warren thought well of it. Dr Cochrane of Nevis employed, with good effects, embrocations of the ol. terebinth.; and Dr Moseley mentions it among the remedies used by the negroes in Jamaica. To keep up the free perspiration, which was induced by the bath and frictions, the patient was covered with blankets, and wore a flannel shirt. At his own request, a fire was added, and sweating carried to a considerable extent. I may here notice, that, from the difficulty of swallowing, and his unwillingness to take any thing at the beginning of the complaint, antimonials, and other sudorifics, were inadmissible. But, in a disease so formidable, and so generally fatal as this, we dare not trust to a single remedy; neither did I intend that sweating alone should be relied on in the instance of Sylvest.

I had remarked, in a recent case of hernia, syncope caused by the administration of tobacco-smoke injections, and it occurred to me, that, if I could produce the same in tetanus, the bowels, which are always constricted, would not only be relieved, but, by its action on the sensorium, the spasms might be overcome. The famed Dr Cullen says, " It is highly probable, that, in this disease, the intestines are affected with the spasms that prevail so much in other parts of the system, and, therefore, that costiveness occurs here as a symptom of the disease," &c.; and, again, " That a relaxation of the intestinal canal will contribute to a relaxation of the spasms elsewhere." Dr Hamilton's recent publication on the effect of purgatives induces a belief of the same. But I am not aware that tobacco-smoke, thrown into the *intestinal canal*, had been practised in the treatment of tetanus previous to this case, and on it I placed my principal reliance. It is true there is mention made by Dr Willis (de motu musculare) of a lady, affected with convulsions, being relieved by the fumes of tobacco blown up her nostrils; and Mr Harris, surgeon of the Magnificent, after Rodney's victory in April 1782, used it in the same way with a tetanic patient that recovered. But, whether the cure of Sylvest is to be attributed to the warm-bath, and oil frictions,—to the artificial heat, and profuse perspiration,—or to the effects of the tobacco fumes;—or whether it might have been owing to a particular state of the patient's constitution,—or an unusually pliable form of the disease, which, by the bye, was not to be expected from the cause,—or from a combination of some or all of these circumstances, I cannot pretend to determine.

I have detailed the case as it occurred. I have given my reasons for walking out of the beaten path; and, from the success of this, I shall feel myself authorized in repeating the experiment.

Before concluding this subject, I may observe, that a boy, about eight years of age, the orphan of a clergyman of this island, and at present in Edinburgh for his education, Daniel Mactair, recovered from the same disease, brought on by cold, under the use of the warm-bath and unctuous frictions, with a very small proportion of laudanum internally. And, a few weeks ago, I knew an infant, of seven days, recover from the trismus infantum, by the warm bath only.

VIII.

Additional Observations on the Treatment of Scrofula. By WILLIAM GOODLAD, Surgeon, Bury, Lancashire.

GENTLEMEN,—I have hitherto been prevented from exciting your attention to a typographical error of some importance in your critique on my Essay on the Diseases of the Absorbent System. I allude to a statement, that the zinc lotion is recommended by me, in the proportion of from half a drachm, to three drachms of the salt, in eight ounces of water, whereas, I have never used more, in the most indolent cases, than a drachm and a half in that quantity; and, if applied in the proportion you mention, it would, I conceive, inevitably produce inflammation, and consequent ulceration of the sac and its coverings.

I have much reason to be satisfied with the favourable opinion you appear to entertain of the practical tendency of my work; but as something like a doubt is expressed, on the efficacy of the sulphate of zinc, when applied in solution to the cavity of, what are called, scrofulous abscesses; and, as considerable scepticism may be allowed, if we only look back to the generally vexatious results succeeding their ulceration, I beg leave to state a few additional facts, through the medium of your Journal, which will, if I mistake not, tend to establish what has been already advanced on this subject. It may, however, be necessary to premise some observations, which, if unnoticed or unknown, might lead a practitioner to contemn the treatment, before he had a fair opportunity of appreciating its effects.

When an inflammatory action continues on the skin, after the formation of matter, or, when the primary source of irritation, which occasioned the enlargement of the gland, and the subsequent abscess, continues to operate as a stimulus, the surgeon will almost certainly be disappointed, if he endeavour to procure adhesion, and it will often be more advantageous to apply a poultice, and promote suppuration. In cases which depend on teething, for instance, unless the tooth have cut the gum, and the inflammation on the skin which covers the abscess have disappeared, previous to its being punctured, the ulcerative action will be continued, and the ulcer must be cicatrized with a granulating surface, and a comparatively large scar.

Another circumstance, which I have not insisted on with sufficient emphasis, is, that the puncture should not be made prematurely, lest a fistulous opening be the consequence. But when the only apparent stimulus existing in the part, arises from distention, and when the integuments have become uniformly thin, so that no thickening remains in the boundary of the abscess, I should rest an entire, firm, and not, I hope, a premature or presumptuous conviction, that, in no instance, will the treatment which I have ventured to recommend disappoint a judicious, nay, even a commonly observant practitioner. Further, a healthy, rather than the most central, or the most depending part, should be chosen for the puncture. It ought not, therefore, to be delayed until the skin itself be affected by the distention of the abscess, for it cannot be expected that any mode of treatment will restore it to a healthy state, if its vital principle be injured, or that a healthy action can ensue from its division, if it have not the power of sustaining its own life.

CASE I.

I was desired by a surgeon at Middlesex, in this county, to visit a young man, eighteen years of age, who had a chain of glandular swellings, extended along the mastoid muscle, from the angle of the jaw to the middle of the neck. The enlargement elevated the neck to a level with the face, and by its pressure rendered his breathing, especially during the night, mastication, and swallowing, very difficult.

Leeches and cold washes were applied without any evident benefit, for, though the platysma myoides prevented the abscesses from pointing, a fluctuation became evident. Fomentations were substituted for the cold application, and I requested to be informed when the integuments became thin, inflamed, or formed into a point. This information was, however, delayed too long, for, on being called to visit him again, the abscess connected with the lowest gland had burst, and discharged about four ounces of healthy-looking pus. I found the edges of the aperture inflamed, and a small white or ash-coloured speck, about the third of an inch distant from it, denoted the commencement of a fresh point of ulceration. The gland was about the size of a small apple, and the patient was free from pain.

Another abscess situated nearer the jaw, and apparently combined with greater enlargement of the gland, would, from its appearance, probably have been several weeks before it pointed; but, as it was desirable that the zinc lotion should be immediately applied, to prevent the lower ulcer from spreading, I determined to evacuate the upper abscess, and endeavour to procure its adhesion by the same means. Three ounces of pus, streaked with blood from the incision, were discharged from it.

A drachm of the sulphate of zinc, dissolved in a pint of water, was kept constantly to the part; the upper abscess united immediately by adhesion; the ulceration in the lower one extended no farther, and its cavity became obliterated also, leaving the space between the ulcerated points to be filled by granulation. These points gradually, but slowly, healed, leaving a cicatrix in the one part, about the size of a pea,—in the other, it was scarcely perceptible.

CASE II.

I was desired to visit the servant of Mr ——— of this town, who had an enlarged gland about an inch from the angle of the jaw, and so closely attached to the bone, that when I first saw it I suspected it arose from caries in the tooth or jaw. Leeches and cold washes were used without effect, for the general health of my patient became disordered, her complaints denoting a strong tendency to consumption. Her strength became much reduced, and the continued application of leeches therefore improper; indeed the gland was little attended to, as the affection in the chest increased.

In two months the tumour was much enlarged, and though in some degree bound down by the platysma-myoides, hung pendulous from the jaw. Poultices and fomentations were used, and, after a tedious process, matter formed and pointed. On the abscess being opened, a solution of the sulphate of zinc was applied, and though the system was labouring under the excitement of phthisis, the sides of the cavity were in a few days united.

Some weeks afterwards a small blush appeared round the punctured part, and was followed by an oozing of a few drops of lymph. This continued some days, when the aperture again healed; and though this action was several times repeated, the opening was never larger than the head of a pin; and when these little collections ultimately ceased, was scarcely perceptible on a minute examination. The enlarged gland also entirely disappeared.

CASE III.

To a man who had an indolent scrofulous ulcer under the chin, I recommended a few months' residence by the sea, but he had been only a few weeks absent, when an abscess appeared rather suddenly under the angle of the jaw, where he had extensive scars left by ulcers, which had existed, and occasionally reopened, from an early period of his life. He returned to his residence immediately, but the diseased skin ulcerated with so small a degree of distention, that the abscess burst before he got home, twelve hours after which period I saw him.

Besides the aperture, near the centre of the abscess, there were two others very near the edge, into each of which a probe could be introduced, and passed from the one opening to the other. Though this disposition to ulcerate shewed great want of vital power in the skin, and was consequently unfavourable to any union taking place, I determined at least to attempt it, and had the satisfaction of seeing the two ulcers on the edge of the abscess heal in a few days. A transparent lymph-like fluid, continued to ooze from the centre opening, though the ulcerated aperture had not extended. A weak solution of the acetite of zinc was daily injected into it, and cold stupes wet in the lotion were constantly applied. The cavity became every day less extensive, and in little more than a fortnight its sides ultimately adhered, and the aperture closed. *

It would, I think, be difficult to produce instances more strikingly illustrative of the superiority of this over every other mode of treating scrofula; and, as it is unnecessary to extend them, I proceed to state, that, when an aperture has been long formed by ulceration before the surgeon is consulted, or these remedies applied, a slough will have formed within the cavity of the abscess, which can only be thrown off by granulation, and, before this is accomplished, the vital power of the skin, and, consequently, its power of reunion, will be destroyed.

If, after a full trial, the surgeon find that no adhesion can be procured, much time will be saved by removing the whole of the ulcerating skin with a knife; and if this operation be carried to the boundary of the abscess, a new skin will, if the patient's health be good, almost immediately shoot forth, particularly if assisted by slight pressure with adhesive plaster, or by the stimulus of a weak solution of a neutral salt.

CASE IV.

In the case of a young man who had disease of the elbow-joint, and an extensive scrofulous ulcer of long standing, under

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* I preferred the acetite to the sulphate of zinc, because it is equally astringent and less stimulating. I am far from ascribing any specific qualities to the latter salt, and have no doubt that other applications might be equally efficacious.

the ear, with the edges of the skin overlapping the ulcer, I endeavoured to produce adhesion, but without effect; and as granulations had risen on the greatest part of the surface of the ulcer, the diseased skin appeared to be the chief preventive to its healing. I removed the skin, therefore, throughout the whole circumference, taking care that no portion was left behind, which was unconnected with the subjacent cellular membrane. A little dry lint was put upon the sore, and a pledget of the ung. lytharg. above it.

At the next dressing, the ulcer was evidently disposed to skin, and it healed rapidly, considering how serious a disease the constitution was already labouring under. I regret to add, that as the patient became reduced it reopened,—he would not consent to the removal of his arm, and is, therefore, dead.

Case V.

A young lady, about twelve years of age, had an ulcer behind the angle of the jaws; combined with considerable glandular enlargement, which had existed three years. Immediately below the ear, was another abscess, which had been burst some time, but the aperture was very small, and the skin not at all discoloured. A small pale and semi-transparent point of granulation protruded through this aperture, which I hoped, by adhering to its sides, would render other treatment unnecessary. But it continued in this state nearly three months, during which time the larger ulcer had healed, the gland connected with it much reduced in size, and another abscess under the sublingual gland, had been punctured, and united by adhesion.

It was particularly desirable, that this patient should be removed from under the eye of a too indulgent mother; who destroyed her daughter's health by excessive attention to it, and particularly by preventing her from taking exercise. I therefore advised her removal to the sea, but, previous to her departure, cut away the loose skin from off the abscess, and thereby reduced it to a simple ulcer, which would heal by any common dressing. I was not disappointed, for in six weeks she returned home, the ulcer had healed rapidly, her general health was established, and she has since continued well.

IX.

Notes of the Case of Thomas Watt, whose extensive Wound, and singular Recovery, were the subject of a Communication from Dr Halliday, in the last Number, p. 140. By JOHN GARRET, Hospital-Assistant to the Forces.

THOMAS WATT, of the Royal Artillery, aged 18 years, was admitted into the General Military Hospital, Williamstadt, on the evening of the 7th February 1814, having received a severe wound on the left shoulder from a cannon-shot. The wound itself had the usual appearance of an extensive gun-shot wound, in which considerable loss of substance had taken place; a sloughy surface of the wound itself, and, surrounding it, the usual appearance of parts which have lost their vitality. It was evident that the greater part of the clavicle had been removed, only about an inch and a half remaining attached to the sternum. The humerus was severely fractured, just above the condyles, as was the last phalanx of the right thumb. The edges of the wound had been brought into contact as much as the nature of the case would allow, by sutures, and straps of adhesive plaster. The great contusion, however, of the surrounding parts, which had since become evident, prevented their being of further service, than in supporting the parts during the long and unavoidable journey to Williamstadt. He complained but of little pain in the wound. The fracture of the humerus, however, and of the right thumb, gave him, on the least motion, excessive pain. The wound was simply dressed this evening, and an opiate given him.

February 8th.—Had no sleep during the night, and this morning was attacked with a most acute pain in his left foot, which, on examination, was red, and painful to the touch. As he had been much exposed to very severe cold in his journey, this latter symptom was attributed to that cause. The foot was directed to be frequently fomented, and a purgative was given, and the opiate repeated at night; the wound much in the same state.

February 9th.—Pain in the foot nearly subsided; a line of separation had formed between the dead parts and the living. Saline medicines were given, with the opiate, in the evening.

February 10th, 11th, 12th.—The treatment was continued nearly the same, regulating the bowels, and continuing the opiate in the evening. Had now become much more tranquil; the sloughs had nearly separated, and the granulations appeared florid and healthy. As the discharge had now become very considerable, he was directed to take the bitter infusion, with tincture of bark, three times a-day, and his opiate in the evening. The dressings were still of the most simple kind. In a few days, the sloughs had quite separated, and exposed a deep wound, extending longitudinally from the spine of the scapula to the axilla, and transversely from the insertion of the pectoralis major across the chest, to about one third the distance from the insertion of the muscle to its origin; the granulations and discharge were healthy, and his general health was good. It would be a tedious recapitulation of nearly the same symptoms and the same treatment, to give a daily narrative of this case. At the end of the month, the wound still retained nearly the same size; the granulating process, however, had gone on rapidly. The great discharge from the wound rendered it necessary to give him a liberal supply of wine, with animal food. Broad straps of adhesive plaster were applied from the arm across the chest; while lint, dipped in an astringent lotion, was applied to the new granulations. Under this treatment, by the middle of the following month, the wound had made great progress to cicatrization. Sinuses, however, had formed in various directions round the wound, the discharge from which was profuse, and pieces of bone were almost daily taken away. About this time, too, a troublesome cough, attended with pain in the chest, and expectoration, increased my apprehensions for the safety of my patient; and the more, as it was accompanied with profuse nocturnal perspirations, and a flushed countenance in the evening. The expectoration however did not appear to be purulent, and his appetite remained good.

The sulphuric acid was now combined with his medicine, the sinuses were injected with tincture of myrrh, and compresses of lint, which were retained in their place by broad straps of adhesive plaster across the chest, were applied in their direction. Under this plan, several of them healed up, and, by the middle of April, his general health was so good, as to enable him to sit up an hour or two during the day. The wound had become now about half of its original diameter; and though his cough was still troublesome, yet his strength improved daily, and his nocturnal perspirations had in a great measure subsided.

April 27th.—This morning, on entering the ward, I found my patient in a more hopeless state than I had yet seen him. He complained of excessive weakness; his countenance was greatly dejected, and his pulse quick and feeble. On dressing the wound, I found that part of the new cicatrix had ulcerated, forming an unhealthy circular ulcer, with livid edges, detached from the general ulcer. He was greatly exhausted with the fatigue of dressing. A stimulating aromatic mixture was directed for him, and he was desired to take freely of his wine. During the day, he was attacked with diarrhœa, which reduced him to such a pitch of weakness, that I despaired of seeing him alive the following morning. Two grains of opium were however given him at night, and a small quantity, combined with his mixture, given at intervals.

April 28th.—Diarrhœa a little abated; pulse still feeble, and is much debilitated. The ulcer has however assumed a more healthy appearance. Cont. med.

By the end of the week he had rallied wonderfully; the sore had nearly assumed its former state, and he was once more able to sit up during the day; his cough had become much less, and not attended with any alarming symptoms. The infus. quassiæ cum tinct. cinchon. ʒij, was directed for him three times a-day.

May 10.—He was able to be removed to another hospital, where I only saw him occasionally. He continued, however, mending slowly, under nearly the same treatment, pieces of bone being from time to time taken away from different parts of the shoulder. By the end of the month his health was so far improved, that he was considered able to be embarked on board the schuyts going with other patients to Antwerp, the establishment at Williamstadt being ordered to be broken up. Since he has been here, very considerable portions of bone have been removed from the scapula, the remaining portion of the clavicle, and the humerus. His health has continued mending since he has been at this station, and is now so far recovered as to be considered quite out of danger. Except a few detached spots of unhealthy granulations, from whence bone is still occasionally taken, the ulcer is quite healed up. His cough has entirely left him, and he has recovered his colour and flesh, and walks out in the air. The fracture of the humerus above the condyles has united, and the last phalanx of the thumb was so shattered as to have come away in different portions. He has, as might be expected, little motion of the arm,—indeed none, except of the fore-arm, which only admits of passive motion, and he cannot yet dispense with the sling. Thus favourably has terminated a case in its commencement entirely hopeless, and in its progress attended with such a combination of unfavourable circumstances as frequently to put an end to all hopes of its ultimate recovery. It was rendered not less interesting from the extreme danger of the wound, than from the great tranquillity and fortitude of mind with which this young man bore so long and seve an illness. Some idea may be formed of his situation, when i is considered, that, for two months, he was under the necessit of being raised up in bed to dress his wound, an exertion rendered particularly painful from the fracture above the condyle on the left arm, and the fractured right thumb. It is impossibl from the nature of the case, to indulge any sanguine expectations that the arm will ever be very serviceable. The wonderfu manner, however, in which the loss of parts of the human machine is frequently supplied, and the age of the patient, ma perhaps allow of indulging a hope, that in time he may have freer motion of his arm than at present.

PART II.

CRITICAL ANALYSIS.

I.

A Practical Treatise on Porrigo, or Scalled Head, and on Impetigo, the humid or running Tetter. With coloured Engravings illustrative of the Diseases. By the late ROBERT WILLAN, M. D. F. R. S. and A. S. Edited by ASHBY SMITH, Member of the Royal College of Surgeons in London. pp. 48, 4to. 1814.

IN a pamphlet of forty-eight pages, we are here presented with the last remnant of the work on cutaneous diseases, on which the late Dr Willan had bestowed a great portion of the labour of his life. It comprises an account only of the first two of the twenty-four genera which remained to be described in the prosecution of his intended arrangement. In consequence of an absurd and improper liberty which the editor has taken, in altering the title adopted by the author, it appears, indeed, at first sight, to be something else than a fragment of the "Description and Treatment of Cutaneous Diseases;" but we are informed in the preface, that it was actually composed by Dr Willan, as a part of the continuation of that work.

With the subdivisions of these two genera into species, and their respective appellations, our readers are already acquainted, through the medium of Dr Bateman's Synopsis of the classification, which has been some time before the public; we shall not, therefore, deem it necessary to enter into much detail in the analysis of the tract before us. The six species of Porrigo are described at length, after some discussion respecting the terms *achor* and *favus*, as well as respecting the Arabian appellations of the disease; and after a similar short preface, relative to the use of the term impetigo, and to the form of the pustules, called *psydracia*, which characterize this genus, the symptoms of the five species, Impetigo *sparsa*, *figurata*, *erysipelatodes*, *scabida*, and *rodens*, are likewise described. In two or three instances, the symptoms and varieties are given somewhat more in detail than by Dr Bateman, especially in the account of Porrigo favosa, and of Impetigo sparsa; but the practical parts of the work are not very ample, nor are the illustrations, borrowed from the ancient writers, so copious as in some of the preceding orders, though characteristic of the learned author's habitual research. With respect to the practical discussions, indeed, there is some reason to doubt, whether these papers had received his finishing hand. A gross deficiency is particularly observable in regard to the treatment of Porrigo favosa; indeed, in this part of the work there is a complete blank. No practical precept whatever is stated by the author, nor a single remedy recommended by him, although he had methodically noticed the mode of treatment for each of the five preceding species under its respective head: his description of this species terminates abruptly with a long quotation from Dr Underwood's treatise on the diseases of children. Has this manuscript, then, never been completed, or has the conclusion of it been lost? The editor has not even noticed, much less attempted to supply, this palpable deficiency: it is obvious, indeed, that either from his inexperience in the arrangement of literary compositions, or his want of a due estimate of the methodical views of the author, whose reputation he has undertaken to uphold, he did not perceive its existence. Had he perceived it, we can have no doubt, from his own assurances of "the vast stores of information" that he is possessed of, that he would have amply supplied the omission. For he favours us with this magnificent detail of his editorial qualifications.

> "The *foundation* of Dr Willan's whole fabric, consisting of a great portion of original notes, and all his case-books, *are* (is) now in the possession of the editor; these he has carefully consulted and compared; the terms of intimacy that subsisted between himself and the author" (though we do not discover that he had ever seen the author till the latter had left England and physic for the fruitless search of health at Madeira) "furnished him with ample means of ascertaining the extent of Dr Willan's indefatigable zeal in all matters of medical research; and who, at length, contemplating the uncertainty of his recovery, *unlocked to the editor*, in moments of friendly intercourse, *the vast stores of information, which he had treasured up* on (in) various departments of medical science, but more especially on cutaneous disorders. (*Pref.* p. vii.)

It is unfortunate, indeed, for the editor, that "moments" are too short for the appropriation of acquirements derived from years of study, even if they consisted of such transferable materials; and that the notes and case-books of others, lying on the shelf, never yet with magical *celerity* transmuted their possessors into learned men. The treasure, in fact, is not in these musty collections, but in the *labour* by which they were accumulated: there is no ready casket of gold in the ground, as we learn in the fable; the wealth is the produce of the toil of *digging* it. And thus these papers appear to have been profitless in the hands of the editor, whose qualifications for his office we should not have been very sedulous to examine, if he had been less forward to magnify and proclaim them:* but neither in the omission above alluded to, nor in the superintendence of the engravings, nor in his notes, do we find any information corresponding with this pompous annunciation.

The engravings, accompanying Dr Willan's work, have always been deemed to constitute a considerable portion of its value, inasmuch as they greatly aided the verbal description, in conveying distinct notions of the various appearances of cutaneous disease, many of which, indeed, words alone are scarcely capable of expressing. In general the engravings, which Dr Willian published, were sufficiently accurate portraits of the diseases which they purported to represent, to give the reader a faithful and correct impression of their character, without making any pretensions to the fine execution of the French artist, (exhibited in the work of Alibert,) who appears at times to be more desirous of making a fine picture, than of faithfully delineating the diseased appearances. Perhaps the most important point to be attained in these representations, is the proper tint or shade of colour; a circumstance which is liable to be varied in the different specimens, which are printed at different times. This point Dr Willan seems to have superintended with considerable care, and, by the combination of the efforts of the pen and the pencil, he produced a very clear notion of many of the varieties of cutaneous disease. We turned with some interest to the engravings accompanying the present publication, in the hope of finding the same accuracy of representation repeated in them; but in this respect we were much disappointed. There has been an obvious want of a superintending eye, to which the appearances of cutaneous disease were familiar, in the printing and colouring of the plates. These are five in number, and three of them contain two figures each. The colouring of the Porrigo larvalis, and of the three figures representing the Impetigo sparsa and figurata, is altogether incorrect, and calculated to mislead, rather than to instruct the student. The black and deeply cellular scab of the Crusta lactea is very unlike in its form and tint, that which we have been accustomed to see; and the representation of Impetigo sparsa (Fig. 1. Plate V.) is, in hue, unlike any thing that we ever observed on the human skin. In all the figures relating to Impetigo, there is no appearance of the characteristic inflammation; and the elevations appear to be vesicular, pimply, scaly, or any thing but pustular. The Porrigo scutulata is pretty well represented, though not in its most common form, in Plate II.; and the pustular character, though not the "honeycomb scab," of Porrigo favosa is strongly expressed in Plate III.: a very indifferent representation of this scab, affecting the chin and mouth, is thrust in at the bottom of the Crusta lactea, in Plate I. and is a reprint of the same imperfect figure, which is to be found in the second Plate of Dr Willan's Treatise on Vaccination.

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* He even took the trouble and expense of inserting the substance of this vaunting preface as a paragraph of news, in a London newspaper. See the Morning Post.

In the remaining portion of the task of editorship, the addition of a few trifling notes, the editor has again evinced the paucity of his profit from the "vast stores" which were "unlocked" in the Atlantic, as well as of his original stock, by the scantiness of his additions in this way, and by having palpably borrowed those which he has made, from a book, which, though it has obviously been the principal object of his studies, he has not once condescended to mention. This half-dozen of notes, which the editor has distinguished by affixing his mark to them, may be found pre-existent in the Synopsis of Dr Bateman. For example, the editor's statement of the opinion of Professor Selle, relative to the Viola tricolor in Crusta lactea, (p. 11.);—his repetition of the synonyms of Haly Abbas, Astruc, Sennert, Sauvages, &c. (in pp. 11. and 15);—his quotations from Dr Hamilton and from Banyer's Pharmacopœia Pauperum (in p. 21.);—his observation on the origin of the epithet *larvalis* (in p. 5.) which, by the way, he, by mistake, ascribes to Dr Willan;—his observations on the ring-worm, and its mode of propagation in schools (in p. 17.);—and on the oil-skin cap (in p. 13.);—are, one and all, very palpable existences in the synopsis just referred to; not always indeed *totidem verbis*, but substantively there. One more note indeed, remains, which was not borrowed thence, nor brought perhaps from Madeira, in which the editor gravely instructs us, that, "In the advanced stages of the complaint, (Porrigo purpurosa) the operation of shaving the head will be facilitated, and the pain attending it diminished, by the previous application of proper remedies." (p. 13.) A very truism! which might have been omitted without disparagement to the editor's wisdom.

On the whole, we do think that the cautious and accurate author of these papers would have been satisfied with such a publication of any fragment of his works, or that he intended this to be so published. The editor, indeed, who, as we learn from a laboured preface, met with him on his voyage to Madeira, takes considerable pains to impress us with the notion, without directly asserting, that he had something like his sanction for the publication. But there is a studied obscurity and some contradiction in his statements, which want the transparency of candour. We are first informed that the editor "landed" at Madeira with Dr Willan; but whether he sailed with him, joined him on the voyage, or was fished out of the sea, is not stated. At all events, "On his arrival at Madeira, Dr Willan devoted himself," he tells us, "to the revision and correction of his manuscripts; a part of which he in a considerable degree remodelled. In this task he engaged the assistance of the editor, and finally placed in his hands the present work, prepared for the press." Now we are immediately afterwards informed, that the request of publication was made subsequently to his death, in consequence of some connection with the family. And the coupling of the MSS. that were *remodelled* with these now published papers, is obviously intended to imply their identity: whereas there is reason to believe that the author had made no revision of his papers on cutaneous subjects for some time previous to his death. For the editor tells us, in the next paragraph, that, "at the time of his death, Dr Willan was engaged in several *literary* undertakings; but, on the subject of cutaneous diseases, strictly speaking, he left nothing, with the exception of the present treatise, in a state to meet the public eye." Now, on referring to a biographical memoir of Dr Willan, published in this Journal, * we find an explanation of this matter, from which it appears that the invalided author was employing himself, and the editor's assistance, only in one of these "*literary* undertakings," and not on the subject of the cutaneous MSS. It is stated, in the memoir just referred to, that Dr Willan had been occupied, for three or four years previous to his illness, in investigating what may be called the antiquities of medicine, especially with a view to illustrate four subjects; namely, the, Ignis sacer; the prevalence of small-pox and other contagious eruptive fevers in ancient times; the leprosy of the middle ages; and the progress of syphilis. It is there also added, that "the dissertations, relative to the two first-mentioned topics, Dr Willan had nearly completed, having *remodelled the second*," (the editor has borrowed the very word), "*by the aid of a friendly amanuensis* during his residence at Madeira." On this subject alone, therefore, we have every reason to believe that the editor's manual assistance was employed by the learned author; and if he had published these papers, respecting which he must have heard much of the author's views and intentions, he would have conferred a greater favour on the public, (who would have been glad to learn the result of the author's extensive researches in regard to these obscure topics,) than by the publication of this fragment, for the superintendence of which his opportunities and information have not very eminently qualified him.

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* See No. XXXII. for October 1812.

But to return to the author. His description of Porrigo favosa we have observed, is given in considerable detail. He has in fact described the eruption under three forms; 1. as it affects the scalp; 2. as it appears on the face; and 3. as it occurs on the trunk and extremities. Under the two first heads, we have the ordinary account of the disease, which, it is admitted, often spreads from the scalp to the face, and in fact often affects both these parts at the same time. The pustules on the scalp, are described as being "large, soft, whiteish, itching, and slightly inflamed at the base. At first they are distinct, and partially distributed, as on the side of the head, or about the occiput." But ultimately the eruption "covers the whole scalp, the pustules in some places remaining distinct, in others becoming confluent, so as to form irregular ulcerated blotches."—

> "In many cases there are among the pustules, small, red, smooth tumours, which desquamate at the top, and very gradually proceed to suppuration, in the same manner as scrofulous tubercles do on the arms, &c. Sometimes large abscesses form near the vertex, or at the occiput, probably originating in lymphatic glands situated there. As soon as they are broken, and begin to discharge freely, the pustular eruption, and small ulcers on the other parts of the scalp, disappear." (p. 24.)

The Porrigo favosa affecting the face sometimes begins there, especially about the lips or upon the chin, and sometimes is an extension of the disease from behind the ears. Children, from one to five years of age, are most liable to have this eruption on the face, and it is usually accompanied with tumid upper lip, inflamed eyes, ulcerated tarsi, &c.

"Adults are occasionally affected with this form of porrigo. The eruption is usually preceded by pains of the stomach, or pains of the head, with giddiness, and general debility. The pustules appear round the mouth and eyes, and on the ears, but more particularly on the cheeks, where they are large, numerous, and much inflamed at the base. In a few days they become confluent, break, and discharge a great quantity of clear, viscid matter, which concretes into thick, irregular masses of yellow scab, encrusting nearly the whole face. In some cases, the eruption is confined to the surface on which the beard grows. The pain and inflammation attending the pustules render the operation of shaving almost impracticable." (p. 28.)

With respect to the eruption of Porrigo favosa on the other parts of the body, the author's observations are all comprised in the following paragraph.

"3. The Porrigo favosa affects the trunk of the body, and the extremities, as well as the face and scalp. It sometimes appears on the arms, back, thighs, ankles, and feet, in distinct blotches, similar to those on the face, and often contemporaneous with them. Deep and obstinate ulcerations are formed at the root of the toes, on the ankles, and heels. The ends of the toes are likewise ulcerated,—the pustules arising even under the nails. In infants less than one year old, I have seen this complaint uniformly diffused over the whole surface of the body, and particularly severe. The eruption consists of achores partially mixed with the favi. It appears in general first on the feet and ankles, and afterwards extends to the abdomen and chest, then round the waist to the arms, neck, and head. There is not much inflammation round the pustules, but after they are broken, a considerable discharge takes place of a clear, unctuous fluid, which gradually concretes into irregular yellow scabs. In some places, as on the abdomen and sides, when the pustules are small and not confluent, the scabs are slight and laminated, and presently separate from the surface. As the eruption proceeds, the inguinal and axillary glands, and perhaps those of the neck, become hard and enlarged: some of them finally inflame and suppurate. Infants suffer much both from pain and itching in this complaint, often for two or three months together, since fresh pustules continually arise on the places from which the scabs have been removed." (p. 29.)

This is followed by the quotation from Dr Underwood, which concludes the subject of Porrigo; not a word being said respecting the method of cure, which the author deemed advisable for the Porrigo favosa, under any of the three varieties above mentioned. There is another deficiency too, which the author would doubtless have considered it necessary to supply, in order to render this tract complete; namely, a representation of the two species, Porrigo *furfurosa*, and *lupinosa*, in the plates, without which the work is certainly unfinished and imperfect. Engravings of the Impetigo erysipelatodes and rodens may not have been considered so necessary; but the absence of these also contributes somewhat to the imperfection of the work.

On the subject of Impetigo, Dr Willan has, on the whole, been less copious, than in the description of the modifications of Porrigo; and the sum of the curative directions is comprised in three short paragraphs, which include also quotations from Dr Underwood, relative to the Impetigo of children, and from Dr Dancer, respecting the impetiginous ring-worm of hot climates. (See pp. 46—48.) The directions consist in recommending for the cure of Impetigo sparsa, tepid ablutions, and the internal use of sulphur; for that of I. figurata, Plummer's pill, with the decoction of elm-bark or sarsaparilla, and ointments of zinc and the superacetate of lead; for the I. erysipelatodes, two or three purgatives at first, and afterwards cinchona in powder, or in decoction with sarsaparilla; and, lastly, for the treatment of I. scabida, emollient washing, and steaming, to remove the crust, and then a covering of oiled silk, with zinc or diluted citrine ointment, Plummer's pill, decoctions of elm, dulcamara, &c. and above all, the waters of Harrogate.

II.

Observations on Pulmonary Consumption. By HENRY HERBERT SOUTHEY, M. D. pp. 174. 8vo. Longman and Co. 1814.

It seems to be almost as hopeless a task to write any thing new respecting pulmonary consumption, as to attempt to arrest its progress by any physical expedient, when the existence of tubercles is already established. Accordingly, we do not find that Dr Southey produces this little volume with any pretensions to furnish us with any additions to the general information possessed by the profession on this subject. The object of his publication is, indeed, no where announced; but after perusing it, we may suppose it to have been his intention to assist the indolent, or those who are too much occupied to read many volumes, by catering for them out of a large mass of books, and bringing together, in a compendious form, a collection of scattered observations, to bear upon the particular points of interest in the history of this mortal disease. Such, at least, is the character of this little compilation; which, though bearing somewhat of the semblance of an academical exercise, is creditable to the industry and discrimination of the author, and exhibits a rapid and spirited sketch of many facts and observations, recorded by a host of writers, whose publications are cited, voyagers and travellers, as well as professional men. The greater portion of these collected observations, relates to the origin and birthplaces, if we may so speak, of the malady in question, and to the various means which have been employed for its prevention and cure; the medical history of the symptoms being detailed with much brevity, together with the appearances on dissection, in two short chapters, and the causes and treatment occupying the remaining chapters of the work.

The only diagnosis, which the author deems necessary for practical purposes, is to mark the distinction "between the consumption of the lungs, which occurs in persons of the *strumous* temperament," and that which, from accidental causes, or as a consequence of other diseases, may attack constitutions of a very different description." p. 4; and among the various characteristics of the scrofulous temperament, which have been pointed out, he considered none more constant than the dilated pupil, and smooth almost transparent skin. Another source of diagnosis, he states, is the usual antecedent of some acute disease, pneumonia, or hæmoptysis, in the cases of consumption which occur in habits, which are not of a strumous character. These indeed, are usually stated as the causes of the different modifications of phthisis; but though violent hæmoptysis seldom occurs in the tubercular consumption; yet how often is the occurrence of slight hæmoptysis among the first symptoms, which alarm the patient, in the early stages of the latter; and how difficult it often is in practice, to mark the change of an acute catarrh into phthisis in persons of scrofulous temperament, and hereditary tendency to the disease. There is no room, however, for any peculiar elucidation of a subject, with which every practitioner is conversant from the commencement to the end of his professional life, and usually finishes where he began; being every now and then surprised by the recovery of an apparent phthisis, and by the occurrence of fatal consumption in persons of vigorous habit, and advancing years. Several modifications of the particular symptoms, in different cases, are stated on the authority of Baillie, Portal, De Haen, Morgagni, Beddoes, &c.

The principal accumulation of the author's research, however, is to be found in the chapter on the "predisposing and exciting causes," in which he has laid under contribution the writings of travellers, naturalists, and physicians, for the production of evidence in illustration of the following queries, with which the chapter opens: namely, "What are the countries, if any, exempt from pulmonary consumption? and where does it occur most frequently? What are the modes of life which prevail in such countries? What classes of the community in our own island seem most, and what least, liable to this malady?" p. 29. From the numerous testimonies to which the author rapidly appeals, in travelling from Iceland, over Russia and the north of Europe, to the Mediterranean, Africa, Asia, and to the western continent and islands, we learn that phthisis occurs in all the northern countries, but is less common in Lapland, Canada, and north Russia, than in Prussia, Germany, and Great Britain; that it is frequent at Paris, and even at Lyons and Montpellier, and especially in the vicinity of the Alps and Apennines; and is well known in Italy, at Florence, Venice, Rome, and Naples, and throughout Spain and Portugal, consumptive patients being sent *from* Lisbon into Alentejo; that in Malta, Sicily, and Greece, it is also common, but is said to be rare in Egypt; and that it appears on the coast of Africa, at the Cape of Good Hope, and Bombay, but less frequently, and is said to be seldom seen in Persia and Hindostan in general by the old travellers. Passing to the western continent, the author traces it, in the writings of travellers, from Greenland to the United States, even to South Carolina; but finds it less common in the West Indies: it appears however at Bermuda, and the Azores, and is not unfrequent among the natives of Madeira. The result of his bird's eye view, in this flight round the world, is thus summed up by the author:

> "From what has been here stated, it may be reasonably doubted, if there is any part of the world in which consumption is unknown. That it occurs less frequently in tropical than in temperate climates, is certain; but it also appears that it is not a common disease in some of the most northern countries. It is to be observed that no distinction has here been made between the different species of pulmonary consumption; and that the authors who have been quoted speak in general terms of every form of the disease. Consumption, as a consequence of active inflammation, external injury, or hæmoptysis, may occur in any climate. Strumous phthisis is by far the most common in this island; consequently all those causes which favour the production of scrofula predispose to consumption." p. 56.

This then leads the author to examine the sources of the scrofulous habit; and from the large proportion of the poor, compared with the rich, who are the subjects of scrofula, according to his belief, he is of opinion, that "deficient food, clothing, and fire," are the principal sources of scrofula, and therefore of consumption. This seems specious; and a host of authorities is quoted for proofs of the exemption of butchers and cat-gut makers, the fish-women of Musselburgh, and the fishermen of Cornwall, from scrofula and phthisis; and for evidence of the increase of phthisis in Scotland, and the vale of Keswick, since "the warm Scotish plaiding was exchanged for the *fine cold* English cloth, and woollen for cotton." Now we question much whether observation will bear us out, even in the first assertion, that scrofula and phthisis are so much more extensively prevalent among the poor, beyond what the greater proportion of that class to the rich, and their necessarily greater exposure to the exciting causes of disease, will explain. If the deficiency of animal food were so active a source of these diseases, should we not find them much more frequent in every other country, than in England, where animal diet appears to be most extensively used? Would not the peasantry of Scotland and Ireland, living principally upon the farinacea and potatoes, be more generally subject to these diseases than the lower classes of the people in England? Do cat-gut makers in reality feed largely on animal substances; or is the exhalation from animal matters wholesome? Our experience would not bear us out in the opinion, that the children of butchers are particularly free from scrofulous affections. One of the most complete instances of a scrofulous family, that we have had occasion to see, occurred in the children of a fat butcher. These sweeping conclusions, which ingenious men are sometimes disposed to make to support particular hypotheses, are indeed scarcely to be depended upon, in consequence of the number of circumstances which operate together to produce a common effect: so that if the fish-women of Musselburgh and the butchers of Bristol, are equally healthy, and exempt from scrofula and consumption, it is scarcely a legitimate inference, that the supply of fish and beef is the principal, much less the sole cause of this exemption. Consumption, and *à fortiori* scrofula, occur in almost every region of the world, as the author has shewn, under every variety of diet and clothing. Are not the vicissitudes of our atmosphere, then, the result of our insular situation in a northern climate, sufficient to account for the greater proportion of phthisical persons to be found in this country? The multiplication of other exciting and predisposing causes in a manufacturing country, will also partly contribute to increase this proportion. The influence of all those occupations which oblige the operators to respire an atmosphere loaded with dust and powdery substances of various quality, is well understood; whence hair-dressers, bakers, stone-cutters, chimney-sweepers, flax and feather-dressers, needle-makers, laboratory-men, cotton-spinners, &c. are peculiarly subject to pulmonary diseases: but the mere sedentary character of many manufactures, also contributes to produce these diseases.

With respect to the contagious nature of consumption, upon which much has been written, and the weight of authorities, Dr Southey observes, is pretty nearly equal, he settles the point by this unanswerable question. "If consumption were indeed communicable by contagion, who in this island could hope to escape?" p. 70. Yet the opinion of its contagious nature is popular in Italy, Portugal, and Spain; so that in these countries, and in Languedoc, it is customary to burn the clothes and furniture of those who die consumptive: in Spain, indeed, this custom is enforced by the civil power. There is a popular tendency to this opinion of contagion in this country; but among medical men there is perhaps no difference of sentiment as to the negative.

In the last and longest chapter, on the "treatment" of consumption, after admitting that cold is unfavourable to scrofulous constitutions, Dr Southey copies Dr Guthrie's account of the habit of the Russians, in order to shew that this people is enabled, by means of warm clothing, warm habitations, and bathing, to bear exposure to an intensely cold atmosphere with impunity; and then proceeds to state that, in this country, "the two great allies of scrofula, cold and want, are not confined to the poor; vanity and folly, in this point, tend to equalize the different classes of society." p. 91. He justly condemns the plan of attempting to *harden* children, by letting them go about half naked, and to drive out *bad humours*, by withholding animal food; and recommends of course the contrary system, especially for the children of strumous parents. He successfully combats the error, also, of supposing that well-heated rooms and warm clothing render the body more sensible to the injurious impression of cold, by a reference to the habits of the Russian and the Laplander, to the practices of manufacturers employed in glass-houses, &c. and to the celebrated experiments of Dr Fordyce and others in a heated room. Indeed every one may have experienced the effect of a warm-bath in cold weather: for if he went shivering to the bath, from the impression of external cold, he would scarcely be sensible of the external temperature on returning. A similar fact is stated by the author. "Every one must have remarked," he says, "that on quitting a warm room, to encounter a frosty atmosphere, less inconvenience is experienced than upon alighting from a cold carriage." Again, "Let two men set out in a mailcoach on a cold night, the one well warmed when he takes his seat, the other shivering from cold; and at the end of a journey of 50 miles, the man who started warm will hardly have felt cold, and the other will never have been warm." p. 97. These familiar facts are perfectly conclusive; and the popular misapprehension upon the subject has arisen, from omitting to make the distinction demonstrated by the late Dr Currie, that the impression of cold is only injurious, when the body, after being over-heated, is rapidly cooling, especially by perspiration, and when exercise has ceased to keep up the activity of the circulation. And Dr Southey affirms, " in all those cases in which I have recommended confinement in rooms heated to 60° or 65°, I have remarked that the attendants most employed in those rooms were less subject to catarrhs than the rest of the family."

The practical inference from these considerations is, that the preventive treatment of the strumous form of pulmonary consumption must consist in an attention to warmth, to the regulation of the functions of the skin, and the bowels, and the use of animal and other nutritious diet, with regular exercise. In robust individuals, who have been attacked with hæmoptysis, the mode of treatment of course must be different. But, the author observes, the medical practitioner has seldom an opportunity of directing the prophylactic regimen, but is most commonly called upon to perform the less promising office of administering medicines to the disease which has actually commenced, to the consideration of which he now proceeds.

After noticing the apparent inconsistency of the different remedies said to be employed with success by different practitioners, which he ascribes to the variety of the disease in different cases, he passes in cursory review before him most of the medicines which have been employed for the cure of phthisis. From *emetics* he believes no permanent benefit is derived, and deems them injurious where hæmoptysis has occurred. The preparations of *iron* he believes to be always injurious, where there is purulent expectoration, and hectic; but useful in chlorotic females of a phthisical habit. The *digitalis* he considers as less frequently beneficial in strumous constitutions, than in the consumption which is preceded by hæmoptysis, and as uncertain in its operation. *Opium* he seems to consider as chiefly useful, in the last stages, when alleviation is all that can be looked for; and deems that, if the sleep which it produces is accompanied by frightful dreams, its operation is more injurious than useful. The *conium* and *hyoscyamus* combined are recommended as substitutes. The *acids*, *neutral salts*, and *balsams*, are briefly noticed; the latter with condemnation, except perhaps in the catarrhal phthisis, or in "that form of consumption in which the purulent expectoration is only an altered secretion from the mucous membrane of the lungs." Blisters are recommended as of essential service in the commencement of the disease; but the author greatly prefers *caustic issues* in a more advanced stage, when the pain is referred to a particular spot, and the object is to keep up external irritation. We may observe, that we have seen the local pains hunted away, by repeated applications of the *tartar emetic* ointment, which is a much less severe and formidable remedy, especially in the incipient stages.

On the subject of removal to a milder climate, Dr Southey points out many objections to that proceeding, even when adopted, as it seldom is, sufficiently early in the disease. In some of the countries of the south of Europe, the heat of the summer months accelerates the fatal termination of the malady. In Lisbon, during the winter, it is often extremely cold, and few apartments are provided with stoves. Madeira is very ill supplied with accommodations, and has proved beneficial to few. The south, or south-east coast of Spain, and particularly Valencia, is considered by our author as the most desirable winter residence for phthisical invalids. It is obvious, however, that the majority of such invalids have not the means of resorting to such a change of residence, and therefore a confinement to apartments, of which the temperature is regulated artificially, is the only succedaneum which can be adopted; upon the same principle as we preserve the tender plants of southern climes by the artificial atmosphere of our hot-houses. We are not adepts in this country, the author remarks, in the art of defending ourselves either from cold or heat, and fail especially in equalizing the temperature of our apartments, there being often a difference of 20 or 30 degrees within the space of a few yards from the fire. Dr Southey asserts that, with double doors and double windows, aided by one of Anderson's patent stoves,* we may regulate the heat of any apartment at pleasure. Most practitioners, we believe, have found it necessary to adopt more or less of this plan in different instances; and few will doubt that many a respite is thus obtained, winter after winter, by its influence, and that occasionally the incipient disease is warded off altogether.

The author is disposed to speak strongly in favour of the *tepid bath*, from the temperature of 90° to 94°, or utmost 96°, used in the middle of the day, in the incipient stages of strumous phthisis; but we know not whether his recommendation is founded on the established value of the remedy in scrofulous affections generally, or from any precise experience of its efficacy in consumption. In recommending *exercise*, both as a preventive and a remedy, the author quotes at length the interesting case of the late Dr Currie, published in Dr Darwin's Zoonomia, in proof that Sydenham's praise of horse-exercise was scarcely exaggerated, and suggests the advantages of a swing, in those cases where this exercise is unattainable, and in weather which renders it impracticable in the open air. Of the effect of *factitious airs*, the author has no experience; but he states the result of some experiments of Dr Fenwick of Durham, respecting the influence of carbonic acid and carbonated hydrogen gas, which were, on the whole, unfavourable to the supposition that any benefit was derivable from these aëriform substances.

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* The stove described and figured in this Journal, Vol. IV. p. 18, as modi- we believe, by Dr Murray, Lecturer on Chemistry in this city.

The essay is concluded by an examination of the opinion lately advanced by Dr Wells; namely, that consumptions are rare in those countries where *agues* are prevalent, and therefore that persons who are threatened with phthisis, should be sent to those marshy districts. Dr Southey controverts the evidence adduced by Dr Wells, and adds some testimonies in opposition to it from the physicians of Essex, which indeed tend considerably to invalidate the ingenious inferences of that author. Some brief observations on diet are added, which seem to amount to this; that, in supporting the strength of a patient in strumous phthisis, we must feel our way, and desist from whatever raises the pulse, and augments the cutaneous heat, soon after it is taken; and that, in those cases of the disease which occur in habits originally robust, or are preceded by hæmoptysis, the diet should be restricted to milk and vegetables. One patient of Dr Gregory's recovered, while living almost entirely upon lemons, and another by indulging in a full diet of animal food, the disease being modified by an opposite temperament in the two individuals. And thus our general principles are reduced at last to the inferences from particular cases and symptoms; and scrofula itself requires a variety of treatment, according to the temperament of the individual whom it attacks. Hence the absurdity of attempting to lay down general rules for the treatment even of strumous consumption, and much more of phthisis in general. We seem therefore to conclude where we began, after a rapid glance of many scattered facts and opinions which have been presented to us before, in a less concentrated view, and to be still left to the guidance of our individual experience and sagacity, in the discrimination and management of each particular instance of the disease.

III.

Observations on the Symptoms and Treatment of the Diseased Spine, more particularly relating to the Incipient Stages; with some remarks on the consequent Palsy. By THOMAS COPELAND, Fellow of the Royal College of Surgeons, and Assistant-surgeon to the Westminster General Dispensary, 8vo. pp. 114. Callow, London, 1815.

EVERY practitioner will admit the justice of Mr Copeland's observation, which applies equally to medicine as to surgery, that "we have more frequently to lament the inefficacy of the art, from the advanced stage in which diseases are presented to us, than from any thing incurable in their nature;" and that "it is no short step in science to discard ill-adapted or superfluous remedies, and to be able to recognize the real nature of a very serious disease, at a period when it is in our power to cure it."—*Preface.* Unfortunately, indeed, the first germs of many chronic diseases are not sufficiently formidable or manifest to excite the alarm of friends, or even of the patient himself, if an adult; nor are they always so distinct as to be detected by an ordinary practitioner, if brought for his inspection. The early detection of such symptoms, however, is the strongest characteristic of true experience and practical skill; although, from the impossibility of measuring the quantity of *prevented* evil, such a diagnosis and anticipation of disease do not obtain the applause and reputation which result from the occasional cure of an established malady. All the writers on the subject of paralysis of the lower extremities, from caries or incurvation of the spine, take up the subject, as the very titles of their books imply, at a period when the disorganization and its consequences have actually taken place. The laudable object of Mr Copeland's treatise is to urge the attention of medical men to the earliest manifestations of the disease, which he is of opinion may be detected, in some cases, by characteristic symptoms, long before the period when caries and incurvation begin. He professes himself, indeed, to be far from satisfied with what he has stated upon the subject. We are persuaded, however, that his hints are valuable, and we think them new and original, and the result of a minute and attentive observation of the phenomena of disease.

A complete analysis of this little work will be scarcely necessary, as the new and leading views of the subject are confined to the diagnosis and treatment of the early stages of the spinal disorder. The first section contains some "general remarks on inflammation of bone and ligament, with relation to those of the spine;" and the second treats briefly of "some diseases capable of producing pressure on the spinal marrow, and consequent paralysis." The diseases mentioned in the latter section, as productive of pressure on the medulla spinalis, are effusions of blood into the theca from external injuries; effusion of fluid from the increased secretions of the inner membrane; and tumours of several descriptions pressing through the space between the articulating processes, which are commonly traced to some accidental violence. These disorders produce all the symptoms of pressure, without any alteration of the external form of the spine. There are also other cases of paraplegia, originating in some affection of the brain, which contribute still farther to the obscurity of the subject.

In the third section Mr Copeland treats of "the early symptoms of the disease of the spine" He justly remarks that the disease, with respect to its *cure*, is an affection of bone or of ligament; but, in respect to *symptoms*, it is a disturbance of nervous function dependent on the spinal marrow. It appears from a quotation, made by the author from the latest work of Mr Pott, that that able surgeon believed, that "no unnatural pressure (was) made on the spinal marrow." But Mr Copeland maintains, that the most prominent symptoms of this complaint arise from the interruption of the nervous energy in parts below the pressure. The debility, inaptitude for motion, speedy fatigue, and relief from rest, of which the patients are early sensible, may partly arise from the diseased condition of the bone or intervertebral substance.

> "But, in most cases," the author affirms, "and more particularly when the disease is in the usual place, the superior dorsal vertebræ, the great characteristic circumstance and symptom, is *a commencing paralysis of the abdominal muscles.* It is surprising how very early in the disease this symptom may be detected, when the attention is directed to it. It is sometimes described as an oppression of breathing, tightness of the stomach, a band tied round the belly, torpor of the abdomen, and by other expressions in different patients. It produces costiveness, and retention of urine in a more advanced stage: in short, in whatever of these symptoms you examine it, some function of the abdominal muscles is recognized to be impeded. No author who has mentioned this disease, has omitted this symptom under some name or other, although it has never, I believe, been fully explained." p. 21.

Mr Copeland recognizes the effects of this commencing paralysis under many points in Mr Pott's admirable description of the disease, which he copies, and in the cases of Dr Jebb and Dr Pierce. He affirms, indeed, that these early symptoms of compression of the spinal marrow, are very generally treated for some other disease, which they most closely resemble.

> "I have seen it called asthma, and prescribed for as such for several months: it is often called dyspepsia, and even diseased liver, from the sense of uneasiness and stricture over the region of the liver and the stomach: sometimes it is taken for a disease of the colon or rectum, from the costiveness and pain that accompany it: the bladder also being unable to perform its office, the cause of this impediment is sought for in the urethra or kidnies." p. 27.

It is true, Mr Copeland admits, that these are common symptoms; nevertheless they should lead us to examine the spine, and thus to distinguish the real seat of the disease, before caries is actually begun. For in this, as in many other instances, he justly observes, as we ascend to the true cause of diseases, and trace the symptoms to their proper source, "several minute but distinct features present themselves to an attentive observer, which were lost in the general idea of pain or uneasiness."

After these judicious observations, which characterize the author as an attentive and acute observer himself, he proceeds, in the following section, to offer his remarks "on the mode of discovering affections of the spine, before the period of incurvation." Although there are some circumstances common to the disease in every part of the spine, especially the relief which is felt on assuming the horizontal posture; yet, he observes, the early symptoms generally will vary according to the part of the spine diseased.

> "If the disease is in the *neck*, there will be pain and difficulty in the rotation and other motions of the head, and the oppressed breathing will be one of the most strongly marked features. If the *back* is the seat of the disease, the oppression at the stomach, as it is called, or the torpor of the abdominal muscles, will be strongly marked. In the *loins*, both these indications are wanting, and the symptoms principally regard the bladder and rectum, whose offices are in a greater or less degree disturbed, according to the degree of pressure made on the nerves which supply them." p. 32.

Mr Copeland admits, however, that the actual seat of the disease in the spine is often determined with difficulty from these early symptoms alone; but the diagnosis is aided, in these cases, by an attention to two circumstances; namely, the greater sensibility to pressure, and to the stimulus of heat, but especially the former. This is to be ascertained by carefully examining with the fingers the whole spinal column, in which the smallest degree of tenderness may be deemed evidence of morbid action within. Where this is not easily ascertained, the susceptibility of the stimulus of heat will often detect the morbid part: thus "a sponge, wrung out of hot water, and carried down the spine, will often give a very acute degree of pain, while passing over the part where disease is going on." This the author discovered by accident, in sponging the bites of leeches over a diseased spine: but his subsequent observations have proved, that the effect is not quite uniform, though often an important auxiliary in determining the diagnosis.

In the concluding section, some brief, but judicious, observations are made upon the method of treatment, founded on the preceding views of the early detection of the disease; and the remainder of the volume is occupied by a detail of cases, partly from the author's own practice, and partly from the publications of Drs Jebb, Charlton, Pierce, Knox, and Maty. The volume also contains two plates illustrative of diseased spine.

It is scarcely necessary, after what we have said above, to add, that this volume is entitled, by the novelty of its views, and the candour and judgment which it evinces, to the attention of the profession.

IV.

Die Keratonyxis, eine neue gefahrlosere Methode den grauen Staar zu operiren. Von W. H. J. Buchhorn, Med. et Chir. Doctor. Magdeburg, 1811.

Depression was for nearly two thousand years the only mode of curing cataract, and was, with little change, performed according to the description and precepts of Celsus, and of the Alexandrian school, until about the middle of the last century, when the operation of extraction was invented by Daviel. Since that time, a competition has existed between the abettors of these two modes of operating, and the latter, by the preference it obtained, had nearly banished the former from practice. Both operations found numerous supporters, and various improvements have been made in each of them, by the simplification of the instruments and attention in the use of them, so that both have been brought to a degree of perfection that leaves little to be altered or improved. That the depression gained many friends and defenders, perhaps arose from its being supposed possible to perform it with greater probability of success, when the number and importance of the accidents that might occur in extraction, were weighed against those that present in depression.

Attentive observation, even in the more early ages, and in more modern times, a minute investigation of the structure of the crystalline lens, as well as the examination after death of the eyes of those who had been at a former period cured by depression, discovered the solubility of the lens. This was so frequently remarked, that several surgeons, and especially Pott, who was a zealous patron of the old method, proposed a peculiar method of depression, resting on this fact. It consisted in the breaking down of the lens in the posterior chamber, by means of the needle introduced in the usual way through the sclerotica, leaving the subsequent solution of the lens to the absorbent powers.

Gleize* was also acquainted with the solubility of the lens. In extracting a cataract, the moment the knife entered the cornea, the patient was seized with convulsions, which prevented the incision of the cornea being completed. To give the patient some chance of cure, he introduced a couching-needle through the puncture, and destroyed as much as possible the structure of the lens and its capsule. In twenty days, the cataract was absorbed, and the patient's sight restored. He afterwards repeatedly performed the operation in the same manner with success. Conradi, perhaps encouraged by Gleize's success, passed a fine lancet-pointed cataract-needle through the cornea, punctured the capsule, and left the lens to be absorbed. Professor Scarpa, also, after breaking down and depressing as much of the opaque lens as possible, directed the remaining fragments to be pushed into the anterior chamber, and left to be absorbed. All these methods rested on the fact of the solubility of the lens in the eye.

In consequence of the favourable result of the above mentioned observations, the author of the treatise now before us was led to make a number of experiments on animals, which he published in 1806, in his inaugural dissertation *De Keratonixide.* His proposed method having been favourably received, and since adopted, by Professors Langenbeck and Himly of Göttingen, the author prosecuted his investigations, and in the present work has given a more full description of his mode of operating, a comparison of it with depression and extraction, and has added the history of some cases in which it was successfully employed.

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* Nouvelles observations pratiques sur les Maladies de l'Oeil et leur traitement. Paris 1786.

Keratonixis (from κερας and νύττω,) *punctio corneæ*, is defined by Dr Buchhorn to be the perforating the cornea by a proper instrument, to perform an operation in the internal part of the eye. Former experiments of a similar nature were confined to cataract, but this operation may be applied to other diseases of the eye, as for the cure of the closed pupil, the evacuation of matter, &c. In the following pages, however, the term is confined to the mode of cure of cataract.

The instrument employed by the author, resembles Scarpa's crooked needle, with this difference, that the curved part is not so long, and the curve not quite so great. The point and sides must be very sharp, and the body of the needle does not increase in thickness from the point, which allows it to be easily moved about in the eye. No preparation of the patient is necessary. Before the operation, the pupil is to be dilated by the solution of the hyoscyamus.

"When the dilatation is sufficient, an assistant raises the upper eyelid with his fingers, the operator lays hold of the needle, as in depression, with the thumb and two first fingers, the other two resting on the temple, nearly on the same level with the eye, as a steadying-point for the hand; the handle of the needle is so far turned backwards, as to lie over the ear of the patient. The point of the needle, which is then situated in the region of the outer angle of the eye, is directed towards the cornea, with its concave part to the cornea, the convexity towards the operator. In this position, he seizes the favourable moment, when the eye is steady, to enter the cornea, and the needle passes without resistance through it, on pressing its point gently at the proper point, moving the end of the handle of the needle from behind forwards, so as to describe the fourth part of a circle."

The point at which the needle is to be entered, varies according to the structure of the eye, and the nature of the cataract. It should not be introduced nearer to the edge of the cornea than about the distance of a line, for fear of injuring the iris; it may even be passed through the centre of the cornea, as no cicatrix is left by it. The author then proceeds:

"When the point of the needle has entered the anterior chamber, part of the aqueous humour flows out along it. But the cornea does not consequently lose its convexity; the pupil merely contracts a little. The point of the needle is then to be directed against the cataract, through the dilated pupil, and the capsule is to be torn in every possible direction. If the lens is *milky*, or of a fluid or soft consistence, the contents of the capsule immediately flow out, mix with the aqueous humour, and render it muddy. The capsule now alone prevents vision. But after it is emptied, it collapses, and, by a slight pressure on its upper part, may be easily depressed into the bottom of the posterior chamber. The muddiness of the aqueous humour is removed by absorption in a few days, and the vision restored.

"If the cataract is *pappy*, *caseous*, or *crumbly*, it is to be broken down in its natural situation as much as possible, without injuring the iris. We endeavour to draw some portions through the pupil into the anterior chamber, as they are more quickly dissolved there than in the posterior, and leave the diminishing, solution, and absorption of the opaque fragments, to nature. This practice resembles that of Pott and Scarpa, only with this difference, that they, as has been noticed, passed the needle through the sclerotica, while I introduce it into the eye through the cornea. Hard cataracts, which I cannot break down by this operation, I depress."*

After this, the needle is to be withdrawn in the same direction it was entered in; and if the cataract is fluid, part of the opaque matter frequently passes out after the needle is removed. The eye-lids are then to be closed, and after a few minutes they are to be opened again, to examine the state of the lens and iris. In some cases, we have seen in the adult the size of the opaque body so much diminished, that the patient was immediately able to distinguish objects held before him. The author says, that he has usually secured the eye-lids, after the operation, with strips of black court-plaster, and over this a compress and circular bandage. In general, we have found it unnecessary to confine the eye-lids, but merely to apply a compress of linen, and pin it to the night-cap. The eye may be opened daily after the operation, and gradually exposed to the light. At first, many of the patients were dismissed three, five, or eight days after the operation; but finding that many neglected the cautions given them of not using the eyes, the author observes, that it will be prudent, even in the most favourable cases, to keep them a fortnight.

In forty cases, seven were attacked by inflammation, and in three eyes the absorption had not taken place after a period of eight weeks. In four of the seven patients, the inflammation was owing to the misconduct of the patients. In one eye the keratonyxis failed, in consequence of the very firm texture of the capsule in a congenital case, which, although torn by the needle, was not absorbed in half a year, and was at last extracted. In a patient affected with amaurosis, previous to the appearance of the cataract, it naturally failed, but restored the patient to the same state as before the lens was affected. The result therefore is:

"Of forty operations, twelve failed. In three eyes, no complete solution has as yet taken place. Seven eyes suffered from inflammation; in five of these the pupil closed, and vision can only be restored in these cases by the formation of an artificial one;—in one the pupil adhered to the cataract, which rose again after being depressed, and one produced no bad effect. One operation failed; because the structure of the cataract prevented its being absorbed. I also reckon the operation on the eye affected with amaurosis among the failures. The remaining twenty-eight succeeded completely, and the patients saw either immediately, or the absorption went on for six or eight weeks, after which time the sight was perfectly restored."

From the failures, we may, with propriety, deduct the three cases of unabsorbed cataract, the case of amaurosis, and the capsular cataract, and one, where, although inflammation came on, active means were employed, and the patient ultimately saw very well. This leaves the proportion of failures six to twenty-eight,—unquestionably a very favourable result to Dr Buchhorn's operation. Professor Langenbeck only met with one case of inflammation out of ten cases, and it was evidently caused by an injury of the iris, from too thick a needle having been used.

With regard to the symptoms consequent to the operation, the author observes that he has sometimes observed vomiting, which however always yielded to appropriate remedies, and was of no consequence. This symptom we have never met with, and would not be led to expect it, as in this operation no nervous tunic is injured, as in the old mode of depression. In young people in whom the operation succeeded, he never observed the distention of a single blood-vessel of the conjunctiva, and, even in patients of fifty or upwards, there was merely a passive congestion of the vessels of the sclerotica, and slight fulness of the conjunctiva, without pain, or other symptoms of inflammation.

Without declaring this method as advisable in every kind of cataract (although he hopes to show that it is so), the author proceeds to mention a few cases in which it is particularly applicable. To these belong,

"1. Cataracts which the operator distinguishes to be *soft* or *fluid*. This is the case where the *keratonyxis* is almost immediately beneficial. The opaque mass flows out, the capsule collapses, is partly depressed, and absorption accomplishes the rest.

"2. *Adhering* cataracts. Their *depression*, as well as the *extraction* of them, is the more difficult the more generally they adhere to the circumference of the pupil. If, according to the old method, force is in any way used in separating them from the iris, iritis and closure of the pupil are the consequence. After the destruction of the capsule by the *keratonyxis*, in which the iris is not injured, the lens is absorbed, if it is not osseous, (and how rare are such cases?) in a longer or shorter period.

"3. Cataracts *in young people*. Children were formerly rarely submitted to either of the former methods before the tenth year, as both were liable to inflammation, which required remedies, and a regimen which children could only be forced to submit to. After the performance of the keratonyxis, on the contrary, no other care of the eye or patient is necessary, than to prevent the child from rubbing the eye, if affected with itchy or stinging pain. The light, even in some cases, need not be kept from the eye, as the pupil becomes clear only in process of the absorption, and till then the remains of the cataract act as a veil before the eye.

"4. *Congenital cataracts.* These, according to the assertion of most surgeons, are always fluid. The effect of the keratonyxis, is sensible at the very moment of the operation; the whole of the mass contained in the capsule flows out.

"5. In persons who have very contracted eyelids, or very *deep-seated* eyes.

"6. In persons who are only blind in one eye. The old methods always occupy a considerable time from the consequent symptoms, and require in extraction the guarding against inflammation, and in the depression the same, with the avoiding violent motion, in order not to make the cataract rise again. All this is seldom necessary in the keratonyxis, which is not followed by inflammation. The patients, although with their eye tied up, follow their affairs as formerly. Perhaps the absorption is even promoted by the motion. Many of my patients from the country, and among these, persons between fifty and sixty years of age, left me on the third day after the operation, and awaited the effect of it at home.

"7. In persons who are weakly, or disposed to convulsions. The depression causes pain in the puncture through the sclerotica, and the subjacent coats; the extraction indeed only perforates the insensible cornea, but occasions painful sensations in the passage of the lens through the pupil. The keratonyxis occasions, as already shewn, no pain, and is peculiarly applicable in persons who are very timid, and are subject to nervous affections.

"8. If one of the old methods fail in the one eye, we ought to choose for the other the keratonyxis, which undoubtedly does less violence to the eye, than the repetition of the depression, or extraction.

"9. In persons, whom we do not wish to expose to the uncertain event of the old methods, whether they will restore the sight or make the person totally blind. As for example, at present in the King of England, where the opposition party, in case of an unsuccessful issue of the operation, raise the doubt, whether the King can still reign? If the keratonyxis fails, that is, if it has no effect, and the lens with its capsule is not absorbed, it may be frequently repeated at short intervals, depending on the state of the eye and of the patient. The eye is never destroyed by this operation; and if it should not succeed, both of the other methods might still be employed."

Such are the cases in which the keratonyxis is principally applicable. But even in cases of firm cataracts which cannot be broken down, the author conceives that it might be employed by simply depressing them. In such cases, however, our experience of both methods would lead us to give a preference to the extraction of the lens, as the keratonyxis, although not liable to all the objections urged against depression, is still exposed to that of the pressure occasioned on the nervous and irritable tunics of the eye exciting inflammation and other symptoms.

We now proceed to give a short comparison of the keratonyxis, with the operations of depression and extraction. In *depression* we pierce first the conjunctiva, a very irritable tunic, so far as it covers the sclerotica; then the sclerotica, after that the choroid-coat full of vessels and nerves, and the ciliary circle; or if the puncture is more forwards, the corpus ciliare, and processes. At last the needle appears in the pupil before the lens, which is depressed, a movement effected entirely in the dark. The patient may perhaps see after it is finished, but the frequent consequence of the injury of so many delicate parts is inflammation, and the exudation of an opaque membrane or closure of the pupil. In extraction, as in keratonyxis, only the cornea, and the less irritable conjunctiva over it, are wounded; but there is a great difference between the extent of the wound, and the injury occasioned by the passage of often a very large lens, besides the discharge of a portion of the vitreous humour, and the risk of the collapse of the eye, irregularity, or even closure of the pupil. In the keratonyxis, on the other hand, the insensible cornea is pierced only at one very small point, the needle passes through the anterior chamber and pupil, and posterior chamber, into the capsule and substance of the lens. All these steps are visible through the cornea; they excite no pain, and no parts are injured which are particularly predisposed to inflammation. Scarcely any confinement is necessary, and the failure of the operation merely consists in its having no effect, and the lens remaining from some cause unabsorbed. The eye remains in the same state as before the operation, and we have it still in our power to perform depression or extraction, or, what is still better, the repetition of the keratonyxis, according to Professor Langenbeck's advice, which we have also frequently practised with success.

We now proceed to consider how long we ought to wait before we conclude that the operation will have no effect. This is very indeterminate, and depends on the age, constitution of the patient, and other circumstances of the case. In children, and in soft or very fluid cataracts, the cure is very rapid, often taking place at the time of the operation. But it is different with hard or capsular cataracts. The author says, that in most of his patients, he has observed the absorption take place in six weeks; and when it did not go on well, he has either repeated the operation, or employed extraction or depression, according to the circumstances that presented during the first operation. He, therefore, conceives himself justified in fixing sixty days as the time in which the greater part of the cataract will be absorbed. Gleize mentions fifty days, and Conradi twelve weeks, and Voelkers a year, as the boundary of the absorption of the lens. From the experience we have had, we are disposed to wait a much shorter period before repeating the keratonyxis. We have observed, that, after two or three weeks, the absorbent process seems stationary; and, after that time, we have been in the practice of again puncturing the opaque body with the needle, or of making a small incision with a cataract knife, and of following Gibson's plan of extracting the fragments through this opening, with the curette or a small hook, and we have often had the satisfaction of thus materially accelerating the cure; so that we would by no means follow the author's advice, of keeping the patient for so long a period in a state of doubt and anxiety.

The author, after mentioning a few disadvantages attending this operation, which appear to us of no material consequence, and equally applicable to depression and extraction, proceeds to relate the history of twelve operations on different kinds of this disease, beginning with the fluid or soft, and ending with the hard and capsular cataract adhering to the posterior surface of the iris. In all these different states the operation was successful, and several minute circumstances are noticed, of importance in operating on these various states.

In our account of this treatise, which appears to us to be the result of actual experiment, and drawn up with care and attention, we have abstained from any comparative view of its merits, opposed to the other methods employed for the cure of cataract. We have always considered that an operator for cataract should be familiar with all the methods of operating, as there are some cases where we would give a preference to extraction, and others where we would expect similar benefit from depression. The keratonyxis, we believe, is not as yet generally employed in this country. From the perusal of the tract before us, and from our own experience in congenital cataract, as well as in the cataract of young people, and even of the adult or aged, we are disposed to recommend its employment as in many respects preferable to depression, or the breaking down of the lens, as now generally practised.

V.

De la Methode Jatraleptique; ou Observations pratiques sur l'Efficacité des Remedes administrés par la voie de l'absorption cutanée dans le traitement de plusieurs Maladies Internes et Externes, et sur un nouveau Remede dans le traitement des maladies Veneriennes et Lymphatiques. Par A. CHRESTIEN, Docteur en Médicine de l'Université de Montpelier, &c. &c. 8vo. pp. 464. Paris, 1811.

WE notice this work chiefly on account of its last section, and shall therefore pass quickly over the preceding ones. Dr Chrestien, following Brera and Chiarenti, has extended considerably the method of administering remedies by applying them to the skin. In the first part of this volume he details many observations on the effects of this mode of administering camphor, opium, colocynth, digitalis, and cinchona, in a great variety of complaints.

He next treats of the good effects of what he calls the resin and resinous extract of cinchona taken internally. These are the extracts prepared by slow evaporation from the tinctures made with rectified and proof spirit, preparations, the one well known in this country under the absurd title of *essential salt of bark*, and the other officinal in a great part of Europe. He administers them in the following manner.

"I triturate a drachm of the resin with the same quantity of salt of wormwood, dissolved in three ounces of water. The solution is not complete, but it is proved to have taken place in part, by the water remaining deeply coloured, after the undissolved part subsides. I commonly employ only half the proportion of carbonate of potass, on account of a sharpness which is sometimes felt in the throat and stomach, when the dose is considerable, and the vehicle in small

> quantity. I put, for adults, a drachm of resin, and half a drachm of salt of wormwood into three ounces of water, and give of this a table-spoonful every two or three hours, according to the indication I have to fulfil, and the time which the paroxysm allows me. The mixture may be made stronger or weaker, according to age and strength of patient. I follow the same procedure with the resinous extract."

This only differs in being more bitter. M. Chrestien supposes the febrifuge virtues of the cinchona to reside entirely in the resin. A few observations follow on the antiemetic virtues of colombo and the use of the common chick-pea, in jaundice and atrabiliary diseases. They were used as coffee, or in decoction, after being roasted.

All M. Chrestien's observations have the same character: they are vague and exaggerated. Not that we accuse him of wilful misrepresentation; but he has evidently that sanguine disposition, which leads him to grasp at every thing which at all seems to favour his opinion, and is apt to deceive himself. His observations are of that kind, which make us desirous of having them repeated, because they are too good to be rejected, and not good enough to be admitted, without confirmation.

The new remedy for venereal and glandular affections, which form the subject of M. Chrestien's encomiums, is GOLD. He was led to think of it long ago, from considering the absurd hypothesis, that mercury cures the venereal disease by its weight; and, as gold is heavier than mercury, of course it should cure syphilis more easily. Clare's method of administering calomel, by rubbing it upon the gums, renewed his desire of trying gold in the same way. His first step was, to obtain it in a state of extremely minute division, and, to effect this, he used different methods; but the description of his processes is exceedingly deficient. His object was, to obtain a powder of gold, which might be rubbed upon the gums, or other absorbing surface; and the preparations which he tried at different times were,

1*st*, Gold, in a state of extremely minute division. This he obtained, by forming it into amalgam with very great care, and then driving off the mercury by means of a very powerful burning-glass; or by subjecting the amalgam to the action of nitric acid, which removes the mercury, and leaves the gold in the state required.

2*dly*, The oxide of gold, procured by precipitating it by potass from its solution in nitro-muriatic acid.

The ammoniacal oxide; but he laid this soon aside, from the danger of its detonating.

The mixed oxide of gold and tin, or purple powder of Cassius, which he prepared in two ways, by precipitating the nitromuriate of gold, by adding filings of tin equal in weight to the gold in the solution, or by decomposing it with the muriate of tin.

3dly, The muriate of gold, obtained by evaporating its solution in nitro-muriatic acid to dryness. This process requires great attention, for if it be not dried enough, it contains too much acid, and if carried too far, the metal is revived.

The muriate of gold and soda. He was led to this preparation, which he seems to prefer, by supposing that the mixture with muriate of soda would correct the causticity and deliquescence of the simple muriate of gold. He describes the preparation of his favourite remedy very indistinctly.

> "It is in the same proportions (equal parts) that I combine the muriate of gold with the muriate of soda. It is not indifferent whether the last be employed in solution or in substance. By procuring the muriate of gold by mixing the two solutions, it has less activity, as experience has convinced me at different times. Perhaps it is the water in which the sea-salt is dissolved, which, by weakening the acid, lessens the energy of the muriate which results from it. Perhaps it depends upon another cause which I have not investigated, because I have seen no advantage in discovering it, as I can dispense with this preparation."

Are we to infer from this, that he mixes the muriate of gold and muriate of soda in substance; or that he adds the muriate of soda in substance to the nitro-muriatic solution of gold, before evaporating it? These various preparations he conceives to differ much in strength.

> "I had carried the dose of gold in extreme division, to the extent of three grains a day, but I was much more cautious in regard to the muriate, and I began by giving only the fifteenth of a grain. Although it was less caustic than that prepared without the addition of muriate of soda, it was still too much so to be employed alone, on account of the impression which it made upon the tongue, where it would have acted as an escharotic, and would have produced local injury without any advantage to the general system. It was therefore necessary to mix it with other substances, which should not decompose it, but would facilitate, or at least permit its absorption. After many trials, I fixed upon a mixture of starch, charcoal and gum lac, although the latter attracted a little of the acid of the muriate, of which I added one grain to two of this substance reduced to very fine powder." p. 341.

In a postscript, he says, that he prefers to this compound vehicle, the orris root in very fine powder, used in the same proportion.

> "I divide this quantity into fifteen doses, then into thirteen, and lastly, into twelve. I have given the tenth of a grain only once or twice, and in ordinary cases, it is seldom that I have occasion to use

more than four grains." "I do not think it superfluous to add, that this muriate, (combined with muriate of soda,) although much less deliquescent than the simple muriate, attracts humidity from the air, when put up in small parcels, but its virtues are not, on that account, sensibly diminished. Nor does it lose after being long mixed with the powder already mentioned, if kept in a well-corked phial." p. 398.

It is to be regretted, that M. Chrestien does not describe distinctly, in any part of his work, the effects of his remedy. We however learn, that it is a specific against the venereal poison. We also learn, and it is not a little suspicious, that it has many of the properties for which empirics in this country are in the habit of extolling their nostrums.

"No season, no temperament, almost no complication oppose the administration of my remedy. There are few cases, when the treatment lasts longer than two months, when the disease is not of very long standing, or when it is not attended by very severe symptoms, or is not complicated, or complications do not arise in the time of administering the remedy, which I have several times observed. I can even assert, that, in the same circumstances, the treatment by mercury, especially rubbed in, would be more tedious. My method possesses the advantage, above all others, of not requiring any thing but sobriety in regimen, and even that need not be very strict, without running the risk of very troublesome occurrences; and it leaves those who are using it, at liberty to attend to their ordinary occupations, in all temperatures of this climate." p. 347. In establishing a comparison between these two muriates, (of gold and of mercury,) it is easy to see, that that of gold is infinitely more active on the system in general, and less irritating to the gums. Clare says, that the friction may be repeated two or three times a day *pro re nata.* I have only twice carried the muriate of gold to one grain in the day, and on one of these occasions I excited a great degree of fever. The friction with corrosive sublimate, sometimes occasions a lancinating pain or excoriation of the gums. The muriate of gold never produces these effects." p. 398. By this agitation of the pulse, I understand the frequency of the pulsations, and the expansion of the artery, which always take place more or less, when I employ my method, especially with the muriate. This excitement, which I consider as indispensably necessary for the cure of the diseases for which I administer gold, when kept within due bounds, is never attended with any notable, or even sensible injury of the functions. The mouth remains clean, the tongue moist, the appetite continues unimpaired, the stools are natural; there occurs only in general an increase of urine or perspiration; but in pushing it too far, I would run the risk of producing general erethismus, or the inflammation of some organ, according to the predisposition of the person; which would not only prolong the treatment, but might excite a new disease, more difficult of cure than that I was attempting to overcome. I adopt as a rule for suspending or modifying my treatment, the supervention of the state next to fever, as indicated by unusual and continued heat of the skin." "Might not the power which my remedy gives me, of producing at pleasure, if I may say so, an excitement of the whole system, render the use of the muriate, by giving in larger or smaller doses, very valuable in the treatment of certain chronic diseases, against which a fever, without affecting the primæ viæ, would be a very powerful remedy?"—p. 399.

He has very properly classed the cases in which the different preparations of gold were employed. The first set was treated by gold divided by mercury. It consists of ten syphilitic cases, and three not syphilitic. Every form of syphilis, recent or confirmed, yielded, in a longer or shorter space of time, to one grain of gold in powder, rubbed into the gums or tongue, once, twice, or thrice a-day, according to circumstances. This might all be. Mercury properly administered, would have effected the same, but the gold also cured in four months, given to the extent of two grains a day, a large tumour under the angle of the jaw, of seventeen years standing; in six months a similar tumour, the size of a small hen's egg, of six years standing; and in twenty months a large *goitre* of twelve years.

The oxide precipitated by potass, and by tin, possessed similar powers against the venereal disease; but the former proved successful in no fewer than seven cases of the scirrhus of the uterus. In some of these the oxide was given internally, in the form of pills, containing each one-tenth of a grain. What are we to think of these cures? Were the patients really affected with *scirrhus uteri?*

The employment of the muriate is detailed in fifteen cases of syphilis, in three cases of glandular enlargement, and in three cases of incipient scrofulous phthisis.

The volume is concluded with five cases of scrofula, in which gold in the form of the muriate, and of the oxide precipitated with tin, was used in combination with the extract of the bark of the Thymeloea Monspeliaca. They were into pills, containing some one grain of the extract, and the 1-120th, and others two grains of extract, and the 1-60th of a grain of the oxide, the former for children, the latter for adults. The cure was begun by giving a single pill daily, and the dose was gradually increased to ten or twelve in the course of the day. The extract sometimes caused vertigo, but never produced any heat in the throat, although the weakest decoction always had that effect.

Our trials, so far as we have carried them, do not confirm M. Chrestien's statement of the activity of gold upon the animal economy. One patient in our hospital has taken in one day 22 grains of the mixed muriate of gold and soda, without the slightest visible effect.

VII. VIII.

The Influence of Tropical Climates, more especially the Climate of India, on European Constitutions, &c. By JAMES JOHNSON, Esq. Surgeon in the Royal Navy. 8vo. Lond. 1813. pp. 554.

The Medical Guide for Tropical Climates, particularly the British Settlements in the East and West Indies, and the Coast of Africa, &c. By RICHARD REECE, M.D. &c. &c. 8vo. London. 1814. pp. 237.

We class these books together, in order to contrast them. Mr Johnson seems to be an enthusiast in his profession; has spent a great part of his life in tropical climates in both hemispheres; has observed well, and practised successfully; and now, in presenting the result of his personal experience to the public, has published a volume abounding in practical knowledge, though sometimes harshly written, which we recommend in the strongest manner to the attention of professional men, and indeed of every person going to tropical climates, on account of the very valuable observations on tropical hygiene, which interest the soldier, the sailor, and the merchant, as much as the physician. With Dr Reece, our readers are well acquainted. At present we have only to state, that he is utterly destitute of experience in the effects and diseases of tropical climates, and that he has, in fact, produced a volume replete with errors, and calculated to do an infinite deal of mischief; and we only trust the following *modest* expectation of its author will never be fulfilled.

"This defect," (the want of a medical guide for tropical climates), "I may with confidence say, it will be only necessary to point out to the Directors of the Honourable East India Company to have redressed; and there is little doubt they will give, on a proper application, their full sanction and authority for this work, intended to be A MEDICAL GUIDE, as well for the direction of the surgeon, as for the use of every individual who is anxious for the care of his health in that climate."

We need not add, that Dr Reece has not omitted this opportunity of puffing all his Dispensaries, tropical, military and continental, his preventive soap, prepared charcoal, essential salt of bark, rhatany root, and all his various publications, acknowledged, anonymous and pseudonymous.

PART III.

MEDICAL INTELLIGENCE.

REPORT of DISEASES treated at the PUBLIC DISPENSARY, near Carey-Street, London, from November 30th 1814, *to February 28th* 1815.

Acute Diseases.

	No. of Cases.		No. of Cases.
Febris	17	Enteritis	1
—— infantum remittens	2	Nephritis	2
Apoplexia	1	Dysenteria	11
Hydrocephalus	3	Rheumatismus acutus	17
Cynanche	3	Scarlatina anginosa	9
Catarrhus	157	Rubeola	16
Pleuritis	3	Variola confluens	1
Peripneumonia	16	Erysipelas	1
———— notha	3	Herpes zoster	2
Pertussis	9	Abortus	1

Chronic Diseases.

	No. of cases.		No. of cases.
Vertigo	7	Tænia	2
Cephalæa	22	Ascarides	2
Asthma and Dyspnœa	7	Hæmorrhois	3
Catarrhus chronicus	65	Constipatio	2
Hæmoptysis	10	Diarrhœa	12
Phthisis	32	Hæmaturia	1
Palpitatio	4	Enuresis	3
Dyspepsia	23	Nephralgia	8
Gastrodynia	12	Dysuria	1
Icterus	3	Cystirrhœa	1
Hepatalgia	2	Hysteralgia	2
Enterodynia	11	Menorrhagia	6
Marasmus	9	Amenorrhœa	5
Tabes mesenterica	1	Leucorrhœa	9
Hæmorrhagia intestin.	1	Rheumatismus chronicus	34

CHRONIC DISEASES continued.

	No. of cases.		No. of cases.
Paralysis - - - - - - -	6	Cachexia syphiloidea - -	1
Hysteria - - - - - - -	7	Strophulus - - - - -	1
Hypochondriasis - - - -	2	Lichen - - - - - - - -	1
Mania - - - - - - - -	1	Psoriasis - - - - - - -	[illegible]
Chorea - - - - - - -	1	Urticaria - - - - - - -	[illegible]
Asthma - - - - - - -	11	Porrigo - - - - - - -	11
Anasarca - - - - - - -	3	Ecthyma - - - - - - -	[illegible]
Hydrothorax - - - - - -	2	Scabies - - - - - - - -	17
Ascites - - - - - - - -	2	Eczema - - - - - - - -	[illegible]
Hydrops. totius corp. - -	4	Aphthæ - - - - - - - -	[illegible]
Scrofula - - - - - - -	1	Lupus - - - - - - - -	1

Although the month of January was frosty for the most part, and during the third week intensely cold, with snow and fogs, yet the winter has been unusually mild; for December was not cold, though damp, and stormy, with heavy gales from the west and south-west; and February has been throughout, not merely temperate, but actually warm.

Nevertheless the usual number of pectoral complaints is to be found in the preceding list, amounting to 300 cases out of 650. Several of them, too, assumed the acute form of catarrhal fever and peripneumony; but the catarrhs, generally speaking, have been much slighter than during the preceding severe winter, and the pulmonic inflammations have been much more manageable, and considerably less fatal. The most unyielding modifications of pneumonia have occurred as the sequelæ of measles, or have supervened upon hooping-cough; under both of which circumstances, the ordinary remedies appear to be less efficacious than in the more simple forms of pneumonic disease.

A few cases of simple fever present themselves sporadically from time to time, and most commonly appear to be referable to the class of *bilious* fevers, *i. e.* to those which owe their origin to gastric derangement, neither resulting from the operation of contagion, nor communicating themselves to more than one individual, even in the ill-ventilated and uncleanly dwellings in which they are found. In one or two cases, however, which were first seen at a late period, and in which the gastric excretions had been neglected, and were in a very much deranged state, the head had become affected, and the disease assumed a low and typhoid type. To those who recollect the numerous cases of typhoid fevers, which called for the relief of dispensaries twelve or fourteen years ago, and the contagion of which was often with great difficulty eradicated from the apartments where it raged, and even seized the same individuals again and again, when they escaped its fatal influence, the great freedom from these fevers, which now exists, even in the most close and filthy alleys in London, is the ground of some surprise. The Fever Institution has, doubtless, contributed much to the purification of many receptacles of contagion, especially within the district superintended by this dispensary, from the situation of the House of Recovery in Gray's Inn Lane. But the diminution of typhus is perhaps too general throughout the metropolis, to be entirely ascribable to the efforts of that useful institution. At the same time, we cannot observe any contemporaneous progress in the diminution of the causes, to which these fevers are usually ascribed, which is at all proportionate to the diminution of fever, or adequate to account for this occurrence. If the scavengers are a little more diligent; if there be a slight improvement in the virtue of cleanliness, and in the taste for fresh air, among all classes of people; if the increasing supply of water afford additional facilities of purification; and if the absence of dearth for several years have contributed to support the public health;—the physicians of dispensaries are well aware that there is still an abundance of dirt, closeness, and starvation, immoveably inherent in numberless districts which they visit, which appear to be as fully adequate as ever to produce and nurture the contagion of fever. Upon the ordinary calculations, it might have been expected, that, during the three months of frost, which occurred last winter, the want of food and fuel must have been so extensively felt among the poor of London, as to render them liable to fever from debility, and to induce them to exclude the air from every crevice for the sake of warmth; yet the population remained free from febrile diseases. The subject is, therefore, involved in considerable obscurity.

Although the efficacy of *oil of turpentine*, as a remedy for the *tape-worm*, has been several times mentioned in this Journal, there can be no impropriety in multiplying evidence in favour of a medicine, which is entitled to the character of a powerful and salutary agent, operating commonly *citò, et tutò*, and, in moderate doses, *not injucundè*. In both the cases of tænia, in the preceding list, *six drachms* of the oil of turpentine, alone and unmixed, were swallowed by the patients, at the dispensary, without the least difficulty or inconvenience, and without producing any immediate effect. In about five hours, it operated as a moderate cathartic on the first patient, a middle aged woman, and produced the discharge of several feet of the worm, which appeared to be dead; although other portions, which had occasionally been evacuated by drastic purgatives, had usually seemed to be alive. In the second patient, a young man of about eighteen, it also operated moderately on the bowels, but without expelling a distinct worm; a great quantity of slimy matter, however, was said to be discharged; he has been subsequently free from the pain in the bowels, which he frequently suffered; and has ceased to pass single joints of the tænia, which he had been accustomed to evacuate. Although the woman has also ceased to expel the joints singly, as heretofore, yet she has had some uneasiness in the belly, similar to what she heretofore experienced. It produced no sickness, nor any affection of the urinary organs, in either of these patients.

The term *cachexia syphiloidæa*, I have borrowed from Mr Pearson, * as conveying with accuracy the two leading characteristics of the disease, a *malus habitus*, *resembling*, in many of the symptoms which accompany it, that state of body which is produced by syphilitic poison;—the *pseudo-syphilis* of Mr Abernethy. Without attempting to enter into any general history of a class of diseases, which is as yet very imperfectly known, it will be sufficient here to state briefly the symptoms of this case. The patient was a married man, about thirty years of age, who had not, according to probable evidence, been the subject of venereal infection, at least for several years. After having experienced a languor and debility, with some loss of flesh, and a sallow complexion, for the space of a few weeks, he was seized with a slight febrile paroxysm, which was followed by a general eruption on the face, body, and limbs, of a papular character, very irregularly distributed, being here and there confluent and clustered in patches, and in other places distinct and distant, and the whole being of a dull brownish-red colour. The throat also became inflamed, and, when examined two or three days after, exhibited a superficial ulceration of the tonsils. The eyes were likewise suffused with redness, and with a tinge of bile. He was for some days in a state of slight fever, with extreme depression of strength, and became at the end of that period completely jaundiced. By means of purgatives, and the nitric acid, the jaundice was speedily removed. The eruption, however, continued, varying occasionally in hue, and being increased at irregular intervals by fresh crops of papulæ; and the debility and languor were also increased at those periods by a fresh accession of the febricula. This complaint continued about five weeks, the whole symptoms, however, becoming gradually less considerable, under the use of the mineral acids and vegetable tonics, especially the sarsaparilla.

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* See his elegant and able work "On the Effects of various Articles of the Materia Medica in the Cure of Lues Venerea."—Introd. p. liv. 2d edit.

London, February 28, 1815. T. B.

Biographical Memoir of the late Henry Reeve, M. D. F. R. S. of Norwich.

It would be an act of injustice to the memory of our late estimable friend and colleague, were we to permit another number of this Journal to appear, unaccompanied by some memorial of his talents and worth, some public testimony of our sincere and deep regret for his premature loss. In attempting to detail the incidents of his short but active life, we shall probably, indeed, incur the imputation of standing forward as the partial eulogists, rather than as the unbiassed biographers of our friend. But we know not how to avoid such an imputation, where simple description will be construed into eulogy. To review the career of those individuals, whose moral excellence and intellectual endowments have been conspicuous in every situation in which they were placed, tends to elevate our views of the human character; and the task, though melancholy, cannot be devoid of interest and improvement.

Henry Reeve was born, in the year 1780, at Hadleigh, in Suffolk, in the vicinity of which his father is in possession of considerable property. After having received the usual preliminary instruction, he was sent, at the age of nine, to a school of great reputation at Dedham, in Essex, under the care of Dr Grimwood, where he entered for the first time upon the study of Latin. Possessing an excellent memory, and uniting great diligence with quickness of apprehension, he made an uniform and rapid progress in this new department of study, and outstripped most of his contemporaries in his classical acquirements; nevertheless, though his acknowledged superiority necessarily raised him to the favour of the master, yet his affability and benevolence, so characteristic of his disposition in after life, averted all envy, and secured him the esteem and attachment of his schoolfellows. Many an exercise, indeed, was shewn up by the indolent or less expert boys, which he had penned in addition to his own. Yet with these habits of industry, he united great activity in the ordinary exercises of school-boys, was distinguished as a cricket-player, and took the lead in some other active pursuits. On one occasion, especially, when an alarm reached the school-room, that a boy had fallen into the river, and was in danger of being drowned, Reeve was the first to reach the brink, and to plunge, at the hazard of his own life, to the rescue of his school-fellow. The confidence which his worthy preceptor placed in the steadiness and humanity of young Reeve, induced him to place under his immediate care an only child, of delicate habit, who was sent to Dedham, at a very early age, and who found a kind protector, as well as a tutor, in his new friend. The success with which Reeve cultivated classical literature, and especially the Latin language, was very considerable, for he subsequently wrote that language with fluency and elegance, of which his thesis, and especially his dedication to Mr Martineau, affords abundant evidence; the point and neatness of the expression in the latter, as well as the sentiments which it contains, were, indeed, highly creditable to his taste and feeling. In addition to his love of the classics, he evinced, while at school, much of that disposition to the examination of natural objects, and to the pursuits of agriculture, which subsequently occupied his attention; and he was sometimes found by his school-fellows when at their play, employing himself in the dissection of a bird or a mouse.

This propensity to the investigation of nature, probably determined his choice of the medical profession, and, on quitting school in the year 1796, he was placed with Mr Martineau, a surgeon of great ability and reputation, at Norwich. Under such a preceptor, who ever retained his warmest esteem and attachment, his assiduity, his zealous spirit of inquiry, and his facility in the acquisition of the objects of his pursuit, could not fail to procure for him a store of fundamental knowledge in his profession, which the earlier studies of the art rarely afford. Accordingly, after passing four years in circumstances thus favourable to his improvement, he went to the university of Edinburgh, in the autumn of 1800, much farther advanced in medical and surgical knowledge, and better prepared to profit by the course of academical studies, which he was about to commence, than the generality of students who repair thither. During the three years, which are required by the statutes of the university, to be dedicated to study by those who graduate there, he was assiduous in his attendance on the different lectures appertaining to his profession, and was led, by his general love of science, to extend his exertions in this way beyond the bounds prescribed by the university to the students of medicine. Thus, in his first year, he attended the valuable course on Natural Philosophy, delivered by Professor Robison; in his second, the interesting and eloquent course of Professor Dugald Stewart, on the subject of Ethics; and, in his third year, he not only became a pupil of Dr Barclay, for the sake of more minute anatomical research than the college then afforded, and of Mr Allen, whose able and elegant course on the subject of Animal Economy, was the select resort of the most ingenious and zealous among the students;—but he also attended, with great assiduity, the lectures of Professor Coventry, on Agriculture and Rural Economy.

Within the pale of the college, no distinctions are conferred to stimulate industry, or to reward merit. The individual who can barely struggle through the ordeal of his final examinations, and he who passes the most rigorous trial with facility, and with an exuberant display of knowledge, equally receive the common degree. Nevertheless, there are, annexed to the system of education at Edinburgh, sources of distinction, and therefore of emulation, not the less calculated to call forth the energies of the students, because they originate and exist exclusively among themselves. In the societies, at which the most respectable of the students meet weekly, for the discussion of scientific topics, the zeal and industry of the members are excited, not only by the prospect of reputation, in consequence of the display of extensive information and ingenuity in the debates, but by the hope of attaining the power and dignities of office, especially of the presidential chair, which are conferred annually on four of the most distinguished members, by their own free ballot. The value, which is attached to these truly honourable stations, may be estimated by the constant exhibition of the titles in the front of the inaugural dissertations. Mr Reeve having joined the Royal Medical Society, entered at once into the discussions which were carrying on, displaying an intelligence and facility of elocution, unusual in so young a member, and in one unaccustomed to speak in a public assembly. The modesty and mildness of his demeanour, his knowledge of the subject, and the little flashes of humour with which he enlivened the gravity of debate, contributed also to obtain for him the early esteem of the members; and in the following year, the second only of his residence at Edinburgh, he was elected to a seat in the presidential chair,—an honour seldom obtained before the third year. He was also elected to the same honourable office, the following year, in a smaller society instituted for the investigation of natural history. His attachment to free discussion, induced him also to become a member of the "Speculative Society," established in the college for the cultivation of eloquence,—a society in which many of the rising ornaments of the senate and the bar at that time exercised their early talents.

In June, 1803, he terminated his active and honourable career at Edinburgh, by taking the degree of Doctor of Physic; on which occasion he evinced his continued inclination to the study of natural phenomena, by selecting for the subject of his inaugural thesis, the nature and causes of the torpidity of hybernating animals. The title of his essay, was "Disputatio inaug. de Animalibus Hyeme Sopitis." We have already alluded to the peculiar neatness of the Latinity, with which his facilities of composition in that language enabled him to clothe this dissertation; and to the good taste and honourable feeling exhibited in the dedication to his much esteemed preceptor, Mr Martineau. Of the contents of this essay, as he subsequently republished it in another form, we shall have occasion to speak hereafter.

Having quitted Edinburgh, Dr Reeve spent the greater part of the two ensuing winters and of the year 1804 in London, for the purpose of extending his professional knowledge, both by learning the opinions of different practitioners, in a new field of inquiry, and by multiplying his own observations among the multitude of maladies, which the various population, and the central attraction of the metropolis, necessarily bring under view. Here he became a diligent attendant on a large hospital, and also on some of the lectures delivered by eminent teachers. He was likewise admitted a member of the Medical Society of London, which holds its meetings in Bolt Court, Fleet Street, and which was at that time the only society in the metropolis, in which the opinions of any of the established practitioners could be heard. He attended its weekly meetings, and, as was his custom, entered actively into the interchange of information, which it was the object of the institution to promote. During this year, while he was half determined to fix his residence in London, he had meditated, together with one of his fellow students, settled in the metropolis, on the want of a good periodical journal, which, by appearing quarterly, might command a sufficient quantity of valuable materials, to be more uniformly interesting than the only monthly medical journal then existing; and Dr Reeve had himself drawn up a *prospectus* of this projected work. At the same moment, however, Dr Duncan, junior, had formed the design of the Edinburgh Medical and Surgical Journal, upon the same plan, and had taken some steps for its commencement; but, having heard of the intention of his two friends, he communicated his proceedings to them, and suggested the mutual advantages which might result from uniting their labours, —a proposal to which the latter immediately acceded. Dr Reeve continued, as long as his health permitted the exertion, to give his valuable aid, in various communications, to which we shall have occasion to allude, as well as in the supply of a considerable share of the anonymous criticism.

During his residence at Edinburgh, Dr Reeve had formed a most intimate friendship, with Mr, now Dr De Roches, a fellow-student from Geneva, of an amiable disposition and considerable talents, who, after his graduation, also continued his medical studies in London. This gentleman being about to return home, in the spring of 1805, Dr Reeve determined to accompany him, for the purpose of visiting as many of the interesting countries of the continent, as were not rendered inaccessible by the occupation of the French armies. They embarked at Harwich, and, having landed at Husum, were compelled, by the presence of the French in the Electorate of Hanover, to go northward, by Lubec, to Magdeburg, whence they travelled through Halle, Leipsic, Weimar, Frankfort, &c. into Switzerland. In the three first places they spent some time, examining the anatomical museum of Loder, and visiting professors Reil, Sprengel, Gilbert, Kuhn, and other learned men. In about six weeks they reached Neufchatel, which, being then under the protection of the king of Prussia, afforded a safe asylum to an English traveller. Here Dr Reeve remained two months alone, with the view of perfecting himself in the French language, and his friend proceeded to Geneva. Here he seems, in consequence of his solitude in a foreign country, to have had a slight degree of the *maladie du pays*, and to have suffered, perhaps for the only time in his life, a considerable depression of spirits; for on his return, at the expiration of two months, Dr De Roches found him pale and thin, and disposed to avoid society and cheerful occupations. The company of his friend, and the commencement of a tour, speedily restored him to his usual hilarity and health. They visited Berne together, and examined its hospitals, of which Dr Reeve published an account in the fifth volume of this Journal. * He was much delighted with a national festival, in one of the most romantic spots of Switzerland, near Interlacken, which was called the *Fête des Bergers*, and at which all the athletic feats of the Swiss peasantry became the subjects of contests and prizes. From thence, under the protection of a little pardonable *ruse de guerre*, he ventured to accompany his friend to Geneva, as an American. His stay, however, was necessarily short, and with much regret on the part of Dr De Roches and his family, they once more separated. Dr Reeve now hastened, in consequence of the advancing season, to the glaciers of Chamouny, and thence passed through many interesting scenes in the Valais and petits cantons, by Zurich, Baden, and Basle, to Schaffhausen; whence he proceeded by Munich to Ratisbon, where he embarked upon the Danube, and made the voyage to Vienna.

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* Edinburgh Medical and Surgical Journal, Vol. V. p. 255.

During this part of his journey in Switzerland, his attention was attracted to the celebrated baths of Leuk, in the Valais, and he published, through the medium of this Journal, * an interesting account of the springs which supply them, and of the surrounding country; describing also the temperature of the baths, the mode of bathing, and the effects of the operation, as well as the diseases which are said to be remedied by resorting thither. In the course of the same journey, his inquiries were directed to the nature of that singular condition of the human economy, which characterizes the Cretins; and he obtained drawings of the skull of one of these unfortunate beings, which he brought to England, and laid before the Royal Society, accompanied by a brief memoir on the subject. † He likewise contributed to this Journal an interesting communication on the same subject, under the title of "Some Account of Cretinism," which was illustrated by two sketches of the skull. ‡ He ascertained that there is no necessary connection between the *goitre*, or tumour of the thyroid gland, and cretinism; nor between either of these diseases and the use of snow-water: but he was disposed to ascribe the disease of the Cretin to the same causes, to which a somewhat analagous malady, the rickets, appears to have owed its origin in this country; namely, bad air, unwholesome food, want of cleanliness, &c.

On reaching Vienna, he determined, in consequence of the disturbed state of the Continent, to make that metropolis his place of residence for the winter. This determination, however, compelled him to break an engagement, into which he had entered previous to his departure from London. During his short stay there, his zeal and information had become so far the object of notoriety, that he was solicited to deliver a course of lectures at the Royal Institution, "on the moral and physical history of man;" and his name was actually announced by the committee among the lecturers of that year.

His residence at Vienna, during the winter of 1805, was the source of much interest; as he not only obtained an extensive introduction to various persons of rank and eminence, but remained there during the occupation of that metropolis by the French, after the defeat of General Mack, and had frequent intercourse with the medical and other officers of the French army. The German language was a leading object of his attention while at Vienna, and he cultivated it with considerable success. A knowledge of this language indeed has become highly important to the medical philosopher, since it has been enriched by the writings of Blumenbach, Reil, Richter, Sprengel, and many other able physiologists and physicians. He likewise availed himself of the opportunity, which a winter at Vienna presented, of prosecuting his researches in regard to the phenomena of torpidity: and for this purpose he kept several marmots, and a hamster, at his lodgings, carefully investigating the state and changes of the functions in these hybernating animals. Several communications to the newly established Edinburgh Medical and Surgical Journal were the result of his observations at Vienna; particularly, an account of the state of Vaccination on the Continent, including a statement drawn up at his request by Dr De Carro;*—two papers, descriptive of the General Hospital, and the School of Medicine at Vienna;†—and an "Inquirer," respecting the then state of Medical Science in Germany.‡

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* Vol. III. p. 150.

† See Philosophical Transactions, Vol.

‡ See Edinburgh Medical and Surgical Journal, Vol. V. p. 31.

Dr Reeve quitted Vienna early in the spring of 1806, and visited Prague, Dresden, and several other cities, which offered objects worthy of attention. At Berlin he remained for some time, having been introduced to the principal literati and other persons of distinction, and embraced the opportunities, which were afforded him, of examining the state of medicine and science in that metropolis. The result of this investigation was briefly detailed in the Edinburgh Journal, together with a list of the diseases which had occurred in the great hospital there, for the space of four years.§ Still obliged by the state of continental affairs to pursue a northern course, he went from Berlin to Hamburgh, and thence to Cuxhaven, where he embarked for England.

For some time after his return, Dr Reeve was undecided as to the place where he should finally settle to practise his profession, balancing especially between London and Norwich. The metropolis, as the great centre of learning, science and wealth, presents many attractions to a professional man of liberal education. It opens before him a wide field for honourable ambition; it abounds with intelligent and polished society; and its very magnitude deprives professional competition of all its personal jealousy and enmity. The road to practice, therefore, appears to be both broad and smooth, in comparison with provincial situations. But having weighed, on the other hand, the extreme uncertainty of ultimate success, and the necessarily long period which must elapse, before any considerable reputation or emolument can be obtained in London; and placing also in the balance the many highly-valued friendships which he had formed at Norwich, the probability of more speedy advancement into practice in that smaller circle, and the more substantial happiness of domestic society, and of that intimate intercourse with a smaller number of real friends, which a provincial residence affords, he decided upon settling at Norwich. Accordingly, in the autumn of this year, 1806, he went to reside in that city. There was another circumstance, indeed, from which also he promised himself more essential happiness than from the splendour of metropolitan reputation or wealth, namely, the completion of an early engagement with a young lady, every way worthy of his attachment, and, in fact, remarkably resembling himself, in the possession of abilities and of solid attainments above the usual acquirement of her sex, as well as in the unaffected cheerfulness, candour, and benevolence of her disposition. This prospect was fully realized, and he ever rejoiced in the determination which fixed him at Norwich. He was married to Miss Taylor in 1807.

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* Vol. II. p. 126.

† Ibid. p. 491, and Vol. III. p. 122.

‡ Vol. IV. p. 69.

§ See Some Account of the Medical Topography of Berlin, Vol II. p. 376.

Although the medical practice at Norwich was greatly preoccupied, especially by Dr Lubbock, and by two or three distinguished surgeons, yet the value of Dr Reeve was speedily recognized by the public, and he was almost immediately appointed physician to the Public Dispensary, on the resignation of Dr Lubbock; and, on the occasion of the death of that much respected friend, in the year 1808, he was appointed his successor in the offices of physician to the Norfolk and Norwich hospital, and to the Bethel.

In the year 1809, Dr Reeve committed to the press his only literary work, (his thesis, and the various papers mentioned in this memoir excepted) which was entitled, "An Essay on the Torpidity of Animals." He had pledged himself, at the conclusion of his inaugural thesis, in 1803, to prosecute the subject farther; and this essay comprises a considerable addition of information, both original and collected, to that which the thesis contained. The third and fourth sections especially, which treat of "the variety of temperature in different classes of animals," with "general remarks on torpidity, on sleep, and on the application of cold in diseases," are replete with curious and interesting observations. It is clearly established, that cold, and want of food, are the principal remote causes of torpidity; but the principal question, by what means certain animals, with a double heart and circulation, are capable of existing, without any respiration, for a considerable period, and yet are recoverable, remains to be determined. The sources of animal temperature are not yet satisfactorily developed. Dr Reeve has reasoned upon the subject acutely; but the subject itself is imperfectly understood, because we are not yet in possession of a sufficient collection of facts from which to deduce any ultimate inference. In October of the same year, on the return of the sick troops from Walcheren, Dr Reeve was appointed temporary physician to the military hospital at Norwich. He was diligent in the investigation of the diseases which came under his care, and kept an accurate register of their appearances, and of the treatment which he pursued.

Not content with the bare performance of the duties required by his official situations, he kindly undertook to promote the progress of some young students connected with the hospital, by communicating to them, at stated periods, at his own house, his opinions on the subject of physiology; and subsequently, in the year 1811, he delivered a public course of lectures on the same subject. This course extended to twenty-five lectures, three of which were given weekly, according to the following plan:—Commencing with a sketch of the natural history of man, the lecturer proceeded to describe the characteristics of animal life, the nature of the muscular fibre, and the phenomena of muscular irritability, and pointed out the analogies and differences between the vegetable and animal world, and between man and the other animals. He then treated of the structure and functions of the brain, and of the distribution and influence of the nervous system; of the faculties and instincts of man and brutes; of the varieties of the organs of sense and motion; and of the senses of sight, hearing, touch, taste and smell, respectively; with incidental observations on ocular spectra, on the deaf and dumb, on the sympathies of different parts, &c. Then followed disquisitions on the vital functions, circulation and respiration, and on digestion, secretion, absorption, and excretion; on animal temperature, and the influence of heat and cold; on the hybernation of animals; and on sleep: and the course was concluded with a view of the growth and gradual decay of the animal body, and of the means of restoration and reproduction; with remarks on population and longevity, on the means of preserving health and prolonging life, and on drunkenness, the temperaments, passions, and antipathies. These lectures were illustrated by various drawings and preparations, which contributed, with the simplicity and perspicuity of the language in which they were delivered, to interest the attention of his audience, and to render a technical subject intelligible and agreeable.

The zeal and activity of Dr Reeve were not, however, limited to professional pursuits. The same spirit of diligence and alertness manifested itself in all the concerns of life. Whenever any measure of public or private utility was proposed, his ardent mind entered quickly and steadily into the project, and his time and means were devoted to its accomplishment; particularly when the amelioration of the condition of the poor and the ignorant was the leading object. While resident in London, his attention had been excited by the establishment of the system of general education, proposed by Lancaster; and in the course of his travels in Switzerland, he was delighted to meet with a similar institution at Iverdun, (so admirably described by Mad. de Stael,) and to have the opportunity of becoming acquainted with its benevolent and enlightened founder, with whom he entered into a minute examination of its plan. Being thus strongly impressed with the practicability of the design, and confident of the extensive benefits likely to result from enlightening the minds of the lower orders of mankind, Dr Reeve was among the first to promote the establishment of a school at Norwich, upon the liberal plan of inviting the children of the poor, of all denominations of religion, to partake of the blessing of knowledge; maintaining that it would be equally unjustifiable to refuse the benefit of education to the members of any sect, or the advocates of any peculiar catechism, as it would be in him to limit his professional assistance to persons who held a particular creed. His activity and personal exertion contributed materially to the establishment and success of this school; for at his house the preparatory meetings of the committee, which organized the institution, were held; and he voluntarily undertook a *census* of the infantine poor in his own district.

The same indefatigable zeal was greatly instrumental in promoting the formation and prosperity of a philosophical society at Norwich. If he did not actually found that establishment, he chiefly contributed to clear away the obstacles which opposed its foundation, and to harmonize and bring together the various combination of age, rank, talent, and taste, in the common cause of philosophical inquiry; and the little code of laws, by which its proceedings were regulated, was principally his work. His regular attendance at its meetings, the alacrity with which he entered into the discussions, the fluent simplicity of his elocution, and the various information which he brought forth, on the different subjects introduced to the society, contributed to the utility and success of the institution.

Thus constantly engaged in the active promotion of every useful and benevolent object, Dr Reeve was at the same time rising steadily in professional reputation; and he had the satisfaction of finding not only that his practice and its emoluments were increasing, but that his skill, assiduity, and kind attention, were highly valued by those who received his aid. In the midst, however, of these gratifying prospects, secure of the ultimate rewards of his talents and acquirements, assured of the permanent confidence and attachment of his fellow-citizens, and happy, as one unusually capable of happiness, in the little domestic circle rising around him, as well as in the intercourse of a few intelligent and esteemed friends, he found his activity and his enjoyments gradually threatened by the symptoms of a slow disease. This insidious indisposition commenced about the end of the year 1811 or the beginning of 1812, and was observed with some alarm by his friends: but he still continued his usual pursuits; and it was not till the month of May, 1813, that he first mentioned the disease, in the course of a regular correspondence with the writer of this memoir. He then described it as a bad state of habit, or one of those conditions of the constitution treated by Mr Abernethy among the "disorders of health." It was characterized by indigestion, weakness, and local pains, which appeared to be sometimes muscular, and sometimes, he thought, in the nerves and bones; by swellings in the lymphatic glands, and an enlargement of the bone of the right arm. At this time he trusted that exercise on horseback, country air, the shower-bath, and a nutritious diet, with an occasional alterative or laxative, (though the bowels had never been out of order) would restore him in a little time to health. And during the summer of 1813 a considerable amendment was in fact produced; but he still suffered from a succession of tumours, which were tedious in suppuration, and slow in healing; and in the course of the winter the malady seemed to be again aggravated. In the following spring, the left knee was seized with pain and swelling, which occasioned so much lameness, as to render walking almost impracticable, and to prevent any communication with his patients except in a carriage; and even that was accomplished with pain and difficulty. In this state of lameness, with two puffy tumours on the scalp, and with a considerable loss of flesh and strength, he determined to quit Norwich and his profession for a time, in search of health; and, having first consulted several of his brethren in London, he resorted to the coast of Sussex, in the beginning of June 1814.

In addition to the lameness of the knee, and to the glandular swellings and tumours of the scalp, which were multiplying and increasing, he had for two or three weeks felt some difficulty of respiration, with a sense of obstruction and huskiness in the larynx, as if from catarrhal inflammation, which also affected the voice. He found his general health improve, and his pains diminish, during his stay at Worthing, by the use of the warm sea-bath, and by the application of a blister and some other local expedients to the knee; but the wheezing increased, and the voice was nearly lost, especially in the mornings. About the 20th of July, indeed, this affection of the trachea became considerably aggravated; the difficulty of breathing increased, and a violent cough now ensued, with a discharge of mucus, sometimes tinged with blood. Hence he was induced to quit the sea-side, and proceeded to Bath. On his arrival at Bath, on the 1st of August, he found the complaint in the wind-pipe less troublesome; but the swelling of the knee was not diminished; it was painful and hot, with an apparent increase of fluid in the cavity of the joint, while the leg was cold and œdematous. Under the direction of Dr Parry, who paid him the most friendly attention, eight leeches were applied to the knee, and afterwards some refrigerant lotions, and he took saline effervescing draughts with squills, and adopted an antiphlogistic regimen. The affection of the trachea somewhat abated, and the voice improved. One of the tumours of the scalp was opened by the knife, and discharged an albuminous fluid, with a good deal of blood, from a wounded twig of the temporal artery; the discharge continued free, and several ounces of blood were lost by a second hemorrhage. Diarrhœa now came on, and an erythematous inflammation attacked the left ear; and both these symptoms continued several days. The muscular pains were relieved; but the tongue became white, and the appetite failed. The knee becoming hot and painful, was again bled with twelve leeches; but these symptoms were not relieved, though the swelling and tension were somewhat diminished: and more decided fever ensued. Leeches were also applied to the vicinity of the trachea; and opium and James's powder were taken with a view to the relief of the dyspnœa and wheezing, which assumed somewhat more severity in morning paroxysms, putting on occasionally the appearance of spasmodic asthma. The febrile symptoms were somewhat abated, when, on the 2d of September, Dr Reeve set off on an easy journey to his father's house at Hadleigh, and passed a few days at Mr Martineau's house, near Islington, for the purpose of rest, and of seeing some of his medical friends in London. He arrived at Hadleigh on the 16th of September. The inflammation of the wind-pipe was now obviously increased, producing much irritation and cough, augmenting the difficulty of breathing, and occasioning a sensation of choking in attempting to swallow. These symptoms were not relieved by blood-letting, the application of leeches, and free doses of hyoscyamus and opium. Restlessness, and agitating dreams ensued; and the respiration being more and more impeded, by the increasing disease in the respiratory tube, he sunk calmly, in the full possession of his faculties, and in his accustomed serenity of mind, under the roof of his father, and in the presence of his afflicted wife and mother, on the 27th of September 1814, in the thirty-fourth year of his age.

The regret which followed this event at Norwich was great and universal. Expressions of sorrow and esteem occupied the columns of the public journals; the Philosophical Society adjourned its subsequent meeting, on the motion of the president, after a warm eulogium had been pronounced on his character by the gentleman who seconded the motion; and his loss was widely lamented in private by those who knew his worth, and both by the rich and the poor who had experienced his kindness and sympathy in the hour of sickness. Indeed, in whatever view his character is contemplated, it was calculated to call forth these testimonials of attachment, and to live in the recollection of all who knew him.

Possessed of a vigorous understanding and great acuteness, he had stored his mind with the riches of literature and science, and had highly cultivated his taste. He quickly appropriated the information which reading, observation, or the most casual conversation threw in his way, and as readily brought it into use when occasion required it. Hence his conversation was various and intelligent, tempered by a mildness of voice and manner, indicative of gentleness of disposition, yet invariably terse and animated, with a considerable propensity to humour, where the subject admitted of it. His unvarying cheerfulness and benevolence of temper, indeed, gave a charm to every action of his life; it extorted the attachment of strangers, and rivetted the love of his friends; for it was uniform and unchanged under all circumstances, in the privacy of the domestic circle, in the more general company of friends, in public meetings, and in the midst of his professional labours and fatigues; and it continued, to a degree truly remarkable, through the whole course of his lingering and painful illness, to the very period of its termination. His unaffected benevolence and never-failing zeal, led him, as we have seen, to incessant exertions in the cause of humanity, even while his own health and comfort were impaired; and it was remarked, that he uniformly appeared to be the least oppressed by his own malady, when he was called upon to administer to the relief of others.

The opinions of the generality of men, respecting the great questions of politics and religion, are modelled by those of the circle in which they happen to move; but those of Dr Reeve were not adopted without much reflection, aided by a sound judgment, and a considerable knowledge of mankind, and were of a highly liberal character. Being always the zealous friend of truth and justice, his wishes were ever on the side of liberty, both civil and religious; he held in abhorrence all systems of fraud and oppression, and detested the attempts of bigotry and superstition, to throw shackles over the human mind. Nevertheless, he held these opinions in the spirit of benevolence, and adhered to no party; being willing to hope, that those who differed from him were actuated by similar motives, and in pursuit of the same objects with himself, however mistaken in the rites which they practised. With such views and dispositions, his duties in private were necessarily performed with the same correctness and zeal, as those of his public and professional life. By the judicious estimate of the true value of domestic happiness, indeed, he had early sacrificed the more captivating allurements of ambition and fame in the metropolis, and in the enjoyment of this choice, he evinced in the character of husband, father, and friend, how much he was destined to promote the felicity of those around him. In short, to borrow the words of a public memorial of him, "his duties in private life were no less happily discharged than those of his profession; his mind was open, generous, lively, simple, and affectionate; and those to whom he was united, as a relative or a friend, will ever turn with melancholy complacency to the remembrance of his faithful and active attachment, of his cheering conversation, and of his pleasing and valuable accomplishments."

In order to avoid any interruption in the narrative of his indisposition, we omitted to state, that, upon the completion of a large building, intended as a lunatic asylum for the county of Norfolk, in the spring of 1814, Dr Reeve was appointed physician to it, in conjunction with Dr Wright,—a measure which was proposed by themselves, with the view of avoiding the pain of a contest, where their claims appeared to be nearly equal. We should also add, that he was elected a Fellow of the Linnean Society, probably about the year 1806, and on settling at Norwich, he became an extra-licentiate of the College of Physicians in London.

In addition to the literary productions already mentioned, several papers were furnished by Dr Reeve to this Journal; namely, an Account of some Cases of Tumour in the breast, Vol. I: p. 159: six of the anonymous papers, entitled "the Inquirer," besides the one already noticed, numbered 14, 17, 18, 20, 21, and 22, and including disquisitions relative to the "conversion of diseases," pointed out by Dr Ferriar; the causes of stone and gravel, the deaf and dumb, diabetes, and some miscellaneous subjects, under the title of "Medicial Gleanings." The eighth volume of the Journal also contains an essay from him on the treatment of chorea, and a brief account of the German universities. We may also state, that in the second volume of the late Dr Currie's "Reports on the effects of water, cold and warm," &c. a communication was published from Dr Reeve, containing an account of the effects of cold affusion, which was used for the first time at Edinburgh, by Dr James Home, in the clinical wards of the Infirmary, while he was a student there.

B.

Died, on the 4th of February, aged 52, JOHN FERRIAR, M. D. Senior Physician of the Manchester Infirmary. The eminent rank which he held in his profession, not only in that town and its immediate neighbourhood, but through a widely extended district of the surrounding country, was founded on long and general experience of the efficacy of his counsels. He was endowed by nature with an acute and vigorous understanding, which he had matured by a life of diligent study, and of careful and well-digested observation, into a judgment unusually correct and prompt in its decisions. The purposes of his sagacious mind were pursued, also, with a steadiness of determination which generally secured their accomplishment; and unexpected difficulties, in the treatment of diseases, he encountered with firmness, and with great fertility of invention. As a professional author he had obtained a high station, and the world is indebted to him for a large fund of valuable knowledge, conveyed in a style, which, for perspicuity, and for manly strength and simplicity, deserves to be proposed as a model to medical writers. His character as a polite scholar will be preserved, in the literary annals of his country, by writings, in which he has displayed correct taste, extensive and various reading, and original views of the subjects of his investigations. In the relations of private life he will long be remembered as a man of inflexible honour and integrity; a faithful and steady friend; and a tender and most indulgent parent.

W. H.

Annual Meteorological Table for Sidmouth, Devon, 50° 41′ N. lat. 3° 13′ W. long. By Dr CLARKE.

13	Thermometer, N. exp.																Barometer.								Weather			Winds.				Rain.
nths.	Highest by Six's register.	Day.	Wind.	Lowest by Six's register.	Day.	Wind.	Mean of the extremes.	Mean of the highest.	Mean of the lowest.	Mean of the highest and lowest.	Mean at 9 morning.	Mean at 2 afternoon.	Mean of 9 A.M. and 2 P.M.	Mean at 11 at night.	Mean of 9 A.M. 2 P.M. & 11 P.M.	Greatest variation in 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the month.	Greatest range in 24 hours.	Fine.	Cloudy.	Wet.	N. and N.E.	E. and S.E.	S. and S.W.	W. and N.W.	Inches and decimals.
n.	49°	6,	S.W.	25°	28,	N.	37°	41°	34°	38°	36°	41°	39°	34°	37°	13°	31,	30.59	N.	8,	29.40	N.	30.14	.41	14	12	5	15	5	4	8	
b.	51	28,	N.W.	31	10,	W.	41	47	39	43	44	47	45	41	44	13	28,	30.57	N.W.	14,	29.22	S.W.	29.93	.43	8	2	18		6	15	10	3.00
rch.	58	29,	W.	28	12,	N.E.	43	51	38	45	48	51	50	41	47	16	7,	30.62	N.	20,	29.74	S.	30.26	.46	21	1	9	10	5	6	14	.35
ril,	59	13,	S.W.	31	2,	N.W.	45	51	39	45	50	51	50	40	47	10	10,	30.69	E.	1,	29.33	S W.	30.16	.58	20		10	7	4	9	13	1.75
ay,	65	29,	N.	40	26,	N.W.	53	59	47	53	57	59	58	50	56	11	31,	30.26	S.W.	14,	29.49	S.W.	29.86	.43	14	5	12	2	8	10	13	2.75
ne,	72	28,	S.	42	17,	S.W.	57	62	48	55	61	62	61	49	57	23	26,	30.34	N.E.	9,	29.61	S.F.	30.08	.30	18	1	11	7	7	14	2	1.22
ly,	70	31,	N.W.	42	7,	S.E.	56	66	48	57	62	66	64	48	59	8	4,	30.39	N.	25,	29.63	S.W.	30.02	.40	18	1	12	4	4	8	15	2.96
ug.	71	2,	S.W.	40	23,	W.	56	67	48	57	63	67	65			8	24,	30.42	S.W.	5,	29.65	W.	30.20	.45	23	1	7	6	5	8	12	.85
pt.	69	17,	S.W.	40	7,	S.W.	55	62	49	56	59	62	61	53	58	13	17,	30.40	S.W.	6,	29.38	S.W.	30.08	.35	18	4	8	7	4	11	8	1.70
ct.	65	5,	S.W.	30	29,	N.E.	48	56	46	51	52	56	54	49	53	20	26,	30.23	N.E.	17,	28.84	S.W.	29.72	.79	13		18	8	2	16	5	3.80
ov.	56	13,	N.W.	32	3,	N.	44	49	41	45	45	49	47	43	45	14	4,	30.44	N.	17,	29.46	N.E.	29.87	.50	11	9	10	9	3	6	12	2.38
ec.	52	18,	S.	25	13,	N.E.	39	43	36	40	40	43	42	40	41	15	27,	30.52	S.E.	2,	29.14	S E.	29.87	.52	20	3	8	12	7	4	8	1.35

ANNUAL RESULTS.

Thermometer.		Wind.
ghest, -	June 28, 72°	S.
west, -	Jan. 28, 25°	N
ean of the extremes,	48°	
ean of the highest,	54°.5	
ean of the lowest,	42°.5	
ean of the two,	48°5	

Barometer.		Wind.
Highest,	April 10, 30.69	E.
Lowest,	Oct. 17, 28.84	S.W.
Greatest range,	Oct. 17, 18, 79	
Mean,	30.01	

Rain for 11 months in dec.

Wind.	Times.	Wind.	Times.
N. N. E.	87	S. S. W.	111
E. S. E.	58	W. N.W.	120
	145		231

Weather.	Days.
Fine,	198
Cloudy,	39
Wet,	128
	365

Annual Meteorological Table for Derby, lat. 52° 58′ N. long. 1° 30′ W. By Mr SWANWICK.

1813.	Register Thermometer, N. exp.										Barometer 170 feet above the level of the sea.								Weather.		Winds.				Rain.
Months.	Days.	Highest in the day.	Wind.	Day.	Lowest in the night.	Wind.	Mean for the day.	Mean for the night.	Mean for the month.	Greatest variation in 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the month.	Greatest variation in 24 hours.	Fair.	Wet.	N. & N. E.	E. & S. E.	S. & S. W.	W. & N. W.	Inches and Decimals.
January	6th	50°	S.	29th	17°	N.W.	37°	31°	34°	13°	25th	30.46	E.	14th	29.38	N. E.	30.00	.41	20	11	5	10	10	6	.30
February	21st	54	S.	4th	31	W.	46	36	41	10	3d	30.40	N.	14th	29.10	S.	29.73	.50	9	19	1	0	17	10	2.14
March	28th	60	W.	31st	30	S. W.	49	35	42	13	26th	30.47	N.W.	21st	29.62	S.	30.14	.41	23	8	5	0	9	17	.72
April	12th	70	W.	3d	29	S. W.	54	38	46	14	13th	30.36	S.	1st	29.05	W.	29.91	.70	22	8	8	3	9	10	.59
May	30th	72	S.	1st	38	N. E.	58	44	51	19	30th	30.10	S.	21st	29.34	S.W.	29.70	.45	10	21	6	3	13	9	3.25
June	2d	76	N.E.	23d	38	W.	65	46	55	12	22d	30.32	N.	9th	29 55	S. W.	30.09	.35	19	11	14	5	6	5	1.17
July	30th	79	S.	9th	44	W.	67	48	58	14	6th	30.30	S.	——	29.30	S. W.	29.78	.79	19	12	3	1	12	15	1.93
August	11th	72	S.	25th	37	N.E.	63	47	55	11	26th	30.28	N.	——	29.50	S. E.	30.02	.30	23	8	8	1	6	16	.56
September	17th	66	S.	10th	36	S.	59	45	52	13	17th	30.31	S.	6th	29.11	S.	30.00	.50	18	12	9	5	12	4	1.42
October	5th	62	W.	19th	26	E.	50	41	46	14	23d	30.24	N. E.	17th	28.93	N.	29.81	.80	15	16	12	4	8	7	5.24
November	20th	52	S	30th	26	S. E.	44	34	39	9	4th	30.51	N.	17th	29.38	N.W.	29.90	.50	16	14	4	4	10	12	1.62
December	18th	48	S W	30th	22	W.	38	32	35	9	11th	30.32	E.	18th	29 25	S. W.	29.83	.42	21	10	9		1[illegible]	8	1.83

ANNUAL RESULTS.

Thermometer.		*Wind.*
Highest, -	July 30th	80° S.
Lowest, -	Jan. 6th	17° N. W.
Greatest variation,	May 28th 29th	19°
Mean for the day,		52.6
Mean for the night,		39.8
Annual mean, -		46.3

Barometer.		*Wind.*
Highest, -	Nov. 4th	30.51 N.
Lowest, -	Oct. 17th	28.93 N.
Greatest range,	Oct.	.80
Annual mean,		29.91
Total rain, in. dec.		20.77

Weather.	*Days.*
Fair, -	215
Wet, -	150
	365

Wind.	*Times.*
N. & N. E. -	84
E. & S. E. -	39
S. & S. W. -	119
W. & N. W.	
	365

Notice of diseased states of the Lungs from Hæmorrhage into the Cavities or interstitial spaces of the Cellular Membrane, with an explanation of a Case formerly communicated by a Physician to Dr Duncan, Jun.

In the Edinburgh Medical and Surgical Journal, Vol. IV. p. 301, anno 1808, I described an occurrence of a disease, with the appearances on dissection, which did not, to my understanding, belong to any of the hitherto distinguished diseases of the pulmonary organs, in which it was seated. Through the channel of that instructing publication, I accordingly sought for the information and opinions of other persons. In a subsequent number of the Journal, an ingenious correspondent gave me pleasure, by bringing forward what he considered as proofs, that the occurrence in question was merely peripneumonic inflammation; but as I had previously given what I considered as proofs that the disease was not one of inflammation, and as no new ones were produced, the matter, in my humble judgment, of course remained undecided. Further experience has taught me, that the disease under inquiry is apparently the hæmoptoe, but in which the blood is effused, not into the bronchial tubes and cells, but only into the cavities of the cellular membrane. Hence the air tubes must be compressed, to no longer admit air; the lungs become dense as other viscera; much increased in specific gravity, and in actual weight of matter, without contraction in bulk; the colour of them was such as might have been expected, and it is, I conceive, easy to interpret all the other phenomena related in the case referred to, without a more particular detail.

The sudden manner, however, in which all the symptoms came on, and the rapidity of the course of the disease, and the immediate exciting agent, viz. a great exertion of the muscles, or a *sprain*,* are, I apprehend, cogent proofs of the disease being a hæmorrhage into the substance of the lungs.

It seems probable, that sudden deaths are not unfrequently occasioned by this hæmorrhage, especially in tuberculous phthisis. Slight cases of this sort may not occasion death, but may produce a partial condensation; and this may occur from time to time, in different parts of the pulmonary organs. I purposely refrain from an explanation of the consequences, except observing that the effused blood may either be entirely absorbed, or be only partially so; in which case adhesion may be accounted for, which obliterate air-vessels, and of course permanent induration will be produced. Adhesion of the cellular substance with the air tubes, is most commonly produced by inflammation, occasioning secretion of coagulable lymph, and the condensed state, so occasioned, is one of the morbid states of pulmonary consumption. In all these cases, there is an addition of matter to the substance of the lungs; but there is a state of condensation, in which there is no addition of matter, viz. that produced by *pressure* of a watery or purulent fluid. Here the lungs, though so dense as to sink in water, or even in alcohol, are capable oftentimes of being inflated like the lungs of the unborn child, and of course there is no adhesion of the cellular substance of the lungs, nor accession of matter.

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* By an error of the press, the muscles are said to be "*straitened*," instead of *sprained*, p. 302, l. 30.

An Account of a Disease of the Larynx and immediately Incumbent Parts. In a Letter to Dr Duncan by a Physician in London.

Although there was no ambiguity with regard to the seat of the disease before death, or even concerning its nature, yet the termination was quite unforeseen, and this example may be the means of preserving life in future.

E. W. ætatis 23 years, a female servant, began to be affected with a disorder producing coughing slightly at times, especially on bodily exercise, with little dyspnœa, and pretty constant wheezing, or, as she expressed herself, the principal complaints were "stoppage in the stomach and the throat," in September last, from no known cause. Pains of the chest, spitting of mucous matter, soreness of throat, and hoarseness, without any appearance of disease visible in the fauces, supervened in the course of a month, which subsisted till death. With some difficulty the patient continued to perform her duties till the latter part of December last; then she applied for relief, the above-related symptoms having become more and more vexatious. Ten ounces of blood were taken from the arm; a purgative was prescribed twice a-week; an anodyne draught at bed-time every night; two grains of ipecacuanha powder thrice a-day; and a tea-spoonful of oxymel was ordered to be swallowed slowly when the cough was troublesome.

The symptoms seemed to be appeased by these medicines, except that the soreness of throat increased, and general swelling of parts about the larynx came on.

During January last blistering-plasters were applied repeatedly round the throat, and then a tuberculated appearance being perceived in the fauces, ten grains of the *pilula hydrargyri* were exhibited every night at bed-time.

None of the symptoms increased, excepting that the voice became much more affected, often producing what was called by the patient and attendants a "whistling;" and at length the breathing was so bad that some degree of orthopnœa manifested itself. There was little or no difficulty in deglutition, nor any constitutional symptoms. On the 7th February the patient paid a visit to some friends, about half a mile distant, with whom she spent the afternoon and evening, till 10 o'clock. She returned on foot; and the air was very cold. About one in the morning feelings of suffocation came on. Medical assistance was immediately called. It was thought proper to administer repeatedly large doses of sulphuric æther, also to bleed, and apply a blister-plaster to the external fauces. It was also in contemplation to perform tracheotomy, but the poor sufferer died about four in the morning, seeming suffocated.

Sectio Cadaveris.—Several very intelligent persons of the profession were present at the examination of the body.

On opening the thorax the lungs exhibited externally no marks of disease. They adhered, indeed, partially and slightly to several parts of the costal pleura, and internally were perfectly sound.

In the pericardium about two tea-spoonfuls of serous liquid were found; otherwise there were no signs of disease of or near the heart, nor of the other parts of the thorax.

The trachea being cut out, the larynx was seen stopt up by thickening of the epiglottis, as well as of the membrane lining the larynx, and by a thick fluid consisting of mucus and coagulated lymph. There was general thickening of the thyroid, arytænoid, and cricoid cartilages. The thyroid *gland*, as it is called, with the muscles attached to the cartilages of the larynx, were all enlarged, and purulent matter was found in the cellular membrane of these muscles. A little pus was seen upon the arytænoid cartilages, and a superficial ulceration.

The surface of the pharynx appeared rough or tuberculated. There was no disease in the bronchial tube, but a little redness in its bifurcation.

The liver was uncommonly extensive, but perfectly sound, as was every viscus of the abdomen.

Remarks.—It seems probable, that, although the passage for air had for many months been considerably contracted, yet merely on that account death would not have been occasioned. It was the increased secretion from the membrane lining the larynx suddenly, produced by the exertion of walking in the cold air, and returning into a warm room, with the drinking, probably, of hot liquids, which blocked up the superior part of the wind-pipe, already very much straitened by organic disease.

As death must seemingly be soon caused in a case like the one before us, there does not appear any just contravening reasons against performing tracheotomy. It may be urged, indeed, that the aperture of the bronchial tube would not cure the disease of the larynx; but it would at least have preserved life until that disease might be cured by the powers of the animal economy, aided by art. This operation has been objected to on account of the hæmorrhagy; but surely the blood-vessels may be avoided from which that danger is apprehended; and the wounds of the trachea produced by suicide seldom prove fatal, unless the large blood-vessels be divided. The now common practice, at the Veterinary College, of tracheotomy, for various diseases of the larynx, shews how harmless it is to make an aperture of a square figure to prevent its closing too soon. With such an aperture, of no inconsiderable size, horses appear little disordered, for they eat, and drink, and sleep, and even perform work, as if no such operation had been instituted.

A Case of Spasmodic Contraction of the Trachea, producing death in eight days time. Mentioned in a Letter to Dr Duncan by a Physician in London.

A young lady, fifteen years of age, was seized, without any assignable cause, with difficulty of breathing, which the apothecary called in imputed to a pulmonary affection, and treated it accordingly. The complaint increased to be most alarming in five days' time. Then I found the patient sitting in a chair, struggling hard for breath, without any symptoms of fever, excepting exceedingly accelerated circulation. Deglutition was performed without any difficulty. The patient was perfectly sensible, and referred the complaint distinctly to the course of the windpipe. Notwithstanding great exertions to save the poor sufferer, by local and general blood-letting, by nauseating dozes of tartrate of antimony (which also produced purging sufficiently), by opium, by blistering plasters to the throat, by the hot-bath, by ethereal liquors, &c. the case proved fatal. For three days before death, the lips were quite purple, and the heart palpitated violently.

Sectio Cadaveris.—The circumstances were such, as to have the body opened, eight hours after death, by two excellent anatomists. On exposing the whole of the trachea, the seat of the disease was seen midway between the larynx and the bifurcation of the trachea, *viz.* one inch and a half in length of this tube was contracted more than two thirds of its diameter; but there was no inflammation, or other disease, of any part of the aspera arteria. The lungs and heart were perfectly sound. The contraction relaxed gradually after the tube was slit, so that the day following the part did not appear any longer contracted, or in a state of disease of any kind.

Extract of a Letter from WM. BRUCE, *Esq. resident at Bushire, to Wm. Erskine, Esq. Bombay, dated Bushire, 26th March* 1813.

MY DEAR SIR,—When I was in Bombay, I mentioned to you that the cow-pox was well known in Persia by the Eliääts, or wandering tribes. Since my return here, I have made very particular inquiries on this subject amongst several different tribes who visit this place in the winter, to sell the produce of their flocks, such as carpets, rugs, butter, cheese, &c. Their flocks, during this time, are spread over the low country to graze. Every Eliäät that I have spoke to on this head, of at least six or seven different tribes, has uniformly told me, that the people who are employed to milk the cattle caught a disease, which, after once having had, they were perfectly safe from the small-pox; that this disease was prevalent among the cows, and shewed itself particularly on the teats; but that it was more prevalent, and more frequently caught from the sheep. Now, this is a circumstance that has never, I believe, before been known, and of the truth of it I have not the smallest doubt, as the person of whom I inquired could have no interest in telling me a falsehood; and it is not likely that every one whom I spoke to should agree in deceiving, for I have asked at least some forty or fifty persons. To be more sure on the subject, I made most particular inquiries of a very respectable farmer, who lives about 14 miles from this, by name Mallila, (whom Babington knows very well,) and who is under some obligations to me. This man confirmed every thing that the Eliäāts had told me, and further said, that the disease was common all over the country, and that his own sheep often had it. There may be one reason for the Eliāäts saying that they caught the infection oftener from the sheep than the cow, which is, that most of the butter, ghee, cheese, &c. is made from sheep's milk, and that the black-cattle yield very little, being more used for draught than any thing else.

A Copy of the Sick Report made to the Commander-in Chief of the North American Station, after Five Months Cruise on the Banks of Newfoundland. By EMANUEL LAZZARETTO, M. D. F. R. S. and Member of the Royal College of Surgeons, London.

THE prevailing diseases this cruize have been catarrhal complaints, attended with fever, headachs, and pain in the breast, which soon was subdued by the usual evacuations, bleeding, laxatives, and diaphoretics. In one particular case the prominent symptoms were excessive tremor of the tongue and of the limbs, hasty speech, vomiting of greenish fluid, picking and fumbling of the bed-clothes, muttering speedily, followed by violent delirium, which recovered by the use of muriatic acid. Towards the latter end of this month scurvy broke out among

Annual Meteorological Table for Derby, lat. 52° 58′ N. long. 1° 30′ W. By Mr Swanwick.

1813.	Register Thermometer, N. exp.										Barometer 170 feet above the level of the sea.								Weather.		Winds.				Rain.
Months.	Days.	Highest in the day.	Wind.	Day.	Lowest in the night.	Wind.	Mean for the day.	Mean for the night.	Mean for the month.	Greatest variation in 24 hours.	Day.	Highest.	Wind.	Day.	Lowest.	Wind.	Mean for the month.	Greatest variation in 24 hours.	Fair.	Wet.	N. & N. E.	E. & S. E.	S. & S. W.	W. & N. W.	Inches and Decimals.
January	6th	50°	S.	29th	17°	N.W.	37°	31°	34°	13°	25th	30.46	E.	14th	29.38	N. E.	30.00	.41	20	11	5	10	10	6	.30
February	21st	54	S.	4th	31	W.	46	36	41	10	3d	30.40	N.	14th	29.10	S.	29.73	.50	9	19	1	0	17	10	2.14
March	2[illegible]th	60	W.	31st	30	S. W.	49	35	42	13	26th	30.47	N.W.	21st	29.62	S.	30.14	.41	23	8	5	0	9	17	.72
April	12th	70	W.	3d	29	S. W.	54	38	46	14	13th	30.36	S.	1st	29.05	W.	29.91	.70	22	8	8	3	9	10	.59
May	30th	72	S.	1st	38	N. E.	58	44	51	19	30th	30.10	S.	21st	29.34	S.W.	29.70	.45	10	21	6	3	13	9	3.25
June	2d	76	N.E.	23d	38	W.	65	46	55	12	22d	30.32	N.	9th	29 55	S. W.	30.09	.35	19	11	14	5	6	5	1.17
July	30th	79	S.	9th	44	W.	67	48	58	14	6th	30.30	S.	——	29.30	S. W.	29.78	.79	19	12	3	1	12	15	1.93
August	11th	72	S.	25th	37	N. E.	63	47	55	11	26th	30.28	N.	——	29.50	S. E.	30.02	.30	23	8	8	1	6	16	.56
September	17th	66	S.	10th	36	S.	59	45	52	13	17th	30.31	S.	6th	29.11	S.	30.00	.50	18	12	9	5	12	4	1.42
October	5th	62	W.	19th	26	E.	50	41	46	14	23d	30.24	N. E.	17th	28.93	N.	29.81	.80	15	16	12	4	8	7	5.24
November	20th	52	S	30th	26	S. E.	44	34	39	9	4th	30.51	N.	17th	29.38	N.W.	29.90	.50	16	14	4	4	10	12	1.62
December	18th	48	S W	30th	22	W.	38	32	35	9	11th	30.32	E.	18th	29 25	S. W.	29.83	.42	21	10	9		11	8	1.83

ANNUAL RESULTS.

Thermometer.		*Wind.*
Highest, -	July 30th	80° S.
Lowest, -	Jan. 6th	17° N. W.
Greatest variation,	May 28th 29th	19°
Mean for the day,		52.6
Mean for the night,		39.8
Annual mean, -		46.3

Barometer.		*Wind.*
Highest, -	Nov. 4th	30.51 N.
Lowest, -	Oct. 17th	28.93 N.
Greatest range,	Oct.	.80
Annual mean,		29.91
Total rain, in. dec.		20.77

Weather.	*Days.*
Fair, -	215
Wet, -	150
	365

Wind.	*Times.*
N. & N. E. -	84
E. & S. E. -	39
S. & S. W. -	119
W. & N. W.	
	365

Programme des Prix proposés par la Société Médico-Chirurgicale de Gand.

La Société Médico-Chirurgicale de Gand, attribuant aux calamités de la guerre, dont toute l'Europe a été affligée, aux inquiétudes continuelles qui ont troublé et détruit le repos si nécessaire à la culture des sciences, qu'on n'a pas répondu d'une manière satisfaisante à la question qu'elle a proposée au concours de l'année dernière; jugeant d'ailleurs que l'objet de cette question est d'éclaircir un point très-difficile et très-important de l'art de guérir, et qui doit ajouter infiniment aux progrès de cet art; a résolu de proposer de nouveau pour sujet d'un prix, consistant en une médaille d'or de la valeur de trois cents francs, quelle décernera dans sa séance publique de l'an 1815, la question suivante:

" Quelles sont les maladies, tant internes qu'externes, qui par leurs apparences, les symptômes dont elles sont accompagnées, ou le siége qu'elles occupent, peuvent être confondues avec les maladies vénériennes: indiquer les signes, les phénomènes, et les moyens par lesquels on peut avec certitude les distinguer de ces dernières affections?"

Les mémoires qui seront destinés à concourir, peuvent être écrits en Flamand, Français, Latin, ou Anglais, et doivent être parvenus, franc de port, avant la fin du mois de Juillet 1815, à Mr Kluyskens, secrétaire-perpétuel de la Société.

On joindra aux mémoires un billet cacheté qui contiendra la devise le nom et le domicile de l'auteur.

Extrait du procès-verbal de la séance du 3 Septembre 1814.
Signé P. E. WAUTERS, Président. J. F. KLUYSKENS, Secrétaire.

Mr ACCUM has in the press " A Treatise on Gas Light," exhibiting a summary description of the apparatus and machinery best calculated for illuminating streets, houses, and public edifices, with carburetted hydrogen or coal gas, together with remarks on the utility, safety, and general nature of this new branch of civil economy.

The treatise will be illustrated with geometrical and perspective designs, exhibiting the structure of the larger gas-light apparatus, now successfully employed for lighting the streets and houses of this metropolis, as well as the smaller apparatus used for lighting manufactories and private establishments.

Dr CLOUGH, physician accoucheur to the St Mary-la-bonne general dispensary, &c. Berner's street, will commence his usual course of lectures on the science and practice of Midwifery, including the diseases of women and infants, on Monday morning, January 9th, at half-past ten, and in the evening at seven.

N. B.—A separate course will be delivered to gentlemen in the army and navy.

We have received communications from Dr NICOL, Mr GIBSON, Mr DUNCAN, Mr WATSON, Mr EDWARDS, Mr FLETCHER, Mr GRIFFITH, Mr MACHELL, and Mr M'COY.

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PART I.

ORIGINAL COMMUNICATIONS.

I.

Description of an Annular Saw; with a plate. By T. MACHELL, Surgeon, Wolsingham.*

WHEN an invention is novel, in both application and principle, it is somewhat difficult to convey a clear and correct idea, by any drawing or description, that may be generally understood; but I am induced to give the following description of my Annular Saw, from the very flattering reception it has met with from the Earl of Stanhope, by whose approval and patronage I have been honoured, as well as that of several surgeons of the highest professional eminence in the kingdom, by whom the annular saw is acknowledged to supersede the use of the trephine, trepan, Hey's saw, bone-nippers, rasp, the mallet and chisel, &c. &c. in all cases where any portion of carious bone demands the interference of our art to remove it. The instruments just enumerated are the best at present in use, in a variety of surgical cases where the common saw cannot possibly be applied; and there are also many cases where even these instruments cannot be used but with the greatest regard to the patient; as in the cure of simple fractures, where there is a want of union, by the bone ends rubbing against each other, from which they become smooth, and are thereby prevented from uniting. We are advised by authors, to lay bare the fractured ends of the bones, and to remove a portion of their callous and smooth edges. This operation has been repeatedly performed, yet I believe with little success, which may be owing to the great extent of muscular surface unavoidably exposed, together with the injury inflicted upon other important parts, in order to apply such instruments as have been hitherto used in performing the operation. The annular saw is considered as well adapted for removing a piece of bone in similar cases with celerity and safety, without injuring any of the large blood-vessels and nerves, that may lie contiguous or immediately in the neighbourhood of the fractured part; *here* the incision must be made, but *need* only be of length sufficient to admit of the soft parts being retracted to the width of about an inch, a little more than the diameter of the bone, for the purpose of applying the saw, which should be, as near as we are able to judge, equal to the bone's diameter. The great length of time so formidable an operation requires in the ordinary mode of proceeding, is, to most surgeons, of little practical importance, as the success of cure is thought to be no way retarded; yet, when the length of time we are engaged in performing an operation is calculated by *minutes* instead of *hours*, this will undoubtedly be a desideratum to the patient.

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* *Note by the Editors.*—We have always declined the publication of any communication which had already appeared, or which, we had reason to believe, was about to appear in any contemporary journal, both to preclude the suspicion of our taking from others what did not belong to us, and that some of our readers might not pay twice for the same article. On the present occasion, we have deviated from that rule, for the following reasons: Mr Machell communicated to us, so long ago as in May 1808, a description and drawing of his saw as he then used it; but, on account of a little want of precision in the drawing, the engraver wished to see the instrument itself, and the communication was laid aside until we should be able to procure it for him. We heard no more from Mr Machell until he lately sent us a *printed* account of his saw, with an engraving of it in its improved state, and soon after the instrument itself. We have examined it, and we think it remarkably ingenious. It is, so far as we know, the first contrivance whereby a circular saw is enabled to cut deeper than its semidiameter, an advantage of incalculable importance

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in the operations for which it is intended. It is also, in reality, less complicated than it appears to be on a superficial examination, and the principle may easily be combined with various subsidiary contrivances to suit various purposes. The instrument we have seen, differs from that represented in the engraving, chiefly in the forceps being somewhat longer, so that they grasp the bone steadily while it is cut.

In the few cases which have occurred in my practice, and in which I employed this instrument, my expectations were fulfilled by finding it manageable, expeditious, and capable of separating bone in situations inaccessible, perhaps, to every other instrument. This instrument is composed of a cutter, which is moved by tooth and pinion, and possesses a superiority over the common saw, in making it cut to any deepness, without using the present saw-like motion. This peculiar *property*, the *shape* of the cutter, with its *vertical* position when worked, are the chief designs that probably claim any merit to invention. To a superficial observer it would seem paradoxical, how a wheel should turn vertically and evenly at the time it was cutting past its centre, as every circular body, when moving truly around, is supposed to be suspended by an axis, and which might naturally be thought would project a little on each side of the cutter, and thus frustrate the intention of dividing bones of a large diameter, imbedded in soft parts. But this is not the case; the instrument represented will remove his doubts, and will be found particularly well adapted for dividing cylindrical bones, and such like hard substances.

Perhaps the following case will demonstrate its decided advantages over any other surgical instrument at present in use, and from this, its application in many similar cases will be at once understood.

A Case of Necrosis.

In 1806, I had under my care J. Taylor, a young man who resided at Witton-le-Wear, county of Durham, and who had become very weak and emaciated from a disease of fourteen years' duration, and had dragged about one of the worst sort of ulcerated legs. At the time I first saw him, he had night perspirations, alarming hectic, and great prostration of strength. On examining the limb, I found it a complete case of necrosis, and determined to remove, if possible, the sequestra, which was enclosed by a case of new bone: this was also perforated by a number of holes of the size of a goose-quill, and from which a copious, sanious discharge was pouring out daily. Neither the head of the trephine, nor Hey's saw, could be admitted, on account of the great destruction to the surrounding soft parts that they would necessarily occasion. Recourse was therefore had to the mallet and chisel, but, on the slightest attempt, the pain was so acute, and the patient having previously refused amputation, now chose rather to die than submit to the operation being completed; so that, for want of other proper means to remove the carious bone, it was at this time deferred, without much hope of ever being able to relieve him. Horrid as these instruments already described may appear, they are the best at present in use in such cases, and are strenuously recommended by our first surgeons, and particularly by Mons. Bousslin. However, after a short absence from my patient, an idea occurred to me that an instrument might be constructed, which would be of considerable advantage in this case, and I delayed no time in communicating it to him, or in getting my design accomplished. When I again saw him, I was prepared to begin the operation, being then under promise to my patient to remove the diseased bone, without making use of such blows as he had before experienced from the mallet and chisel, and which, with those instruments, could not possibly be avoided. I first cleared away, for the space of an inch and an half by about an inch, the integuments and periosteum, which had become much thickened, down to the bone at the upper part of the leg, when the new bone, to the dimensions described above, was very easily sawed through with the Annular Saw, which exposed the upper end of the sequestra: this part was cut off transversely, again divided longitudinally, and then extracted in two pieces; the remaining part was laid hold of by the common forceps, drawn up to the top of the opening, and then sawed off. This was repeated thrice, and the whole sequestra, being seven or eight inches in length, was sawed into pieces in the middle of the leg, and extracted by an opening in the integuments and new bone, of the above dimensions. The violent irritation which the diseased bone had occasioned, obliged him to have constant recourse to opium with good effect, and in the space of six months he was so far recovered, as to be able to walk without a stick, and was restored to perfect health.

Dr Clanny, a most respectable physician of Sunderland, was present during the operation.*

Explanation of the drawing of Machell's Annular Saw.

THIS instrument may very properly be said to consist of two distinct parts, one of which I have denominated by the term of saw, to distinguish it from the other part, which is named the conveyance, purposely that the following description of my annular saw may be perfectly understood: The first of these parts is represented by D, and is of the form of a ring, being made of steel, well tempered, to penetrate any hard bony substance without damage to the fine points of the teeth that are on the outer edge, and act as a saw. On this edge there are also wheel-teeth,

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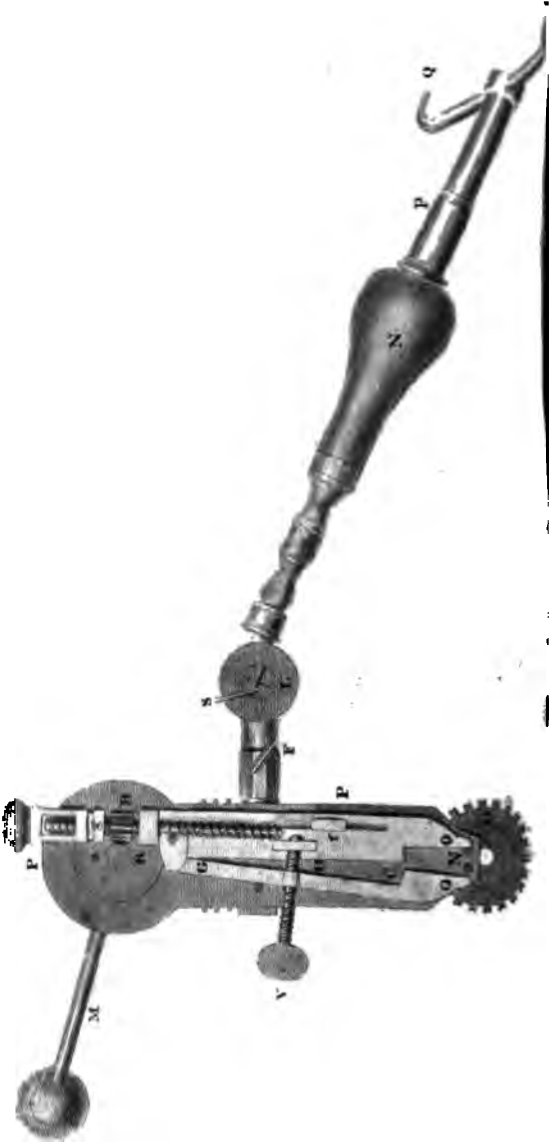
* We think it unnecessary to reprint the certificates of Dr Clanny, Sir Everard Home, Messrs Charlton, Doratt, and Ingham.—*Editors.*

[illegible] Annular Saw.

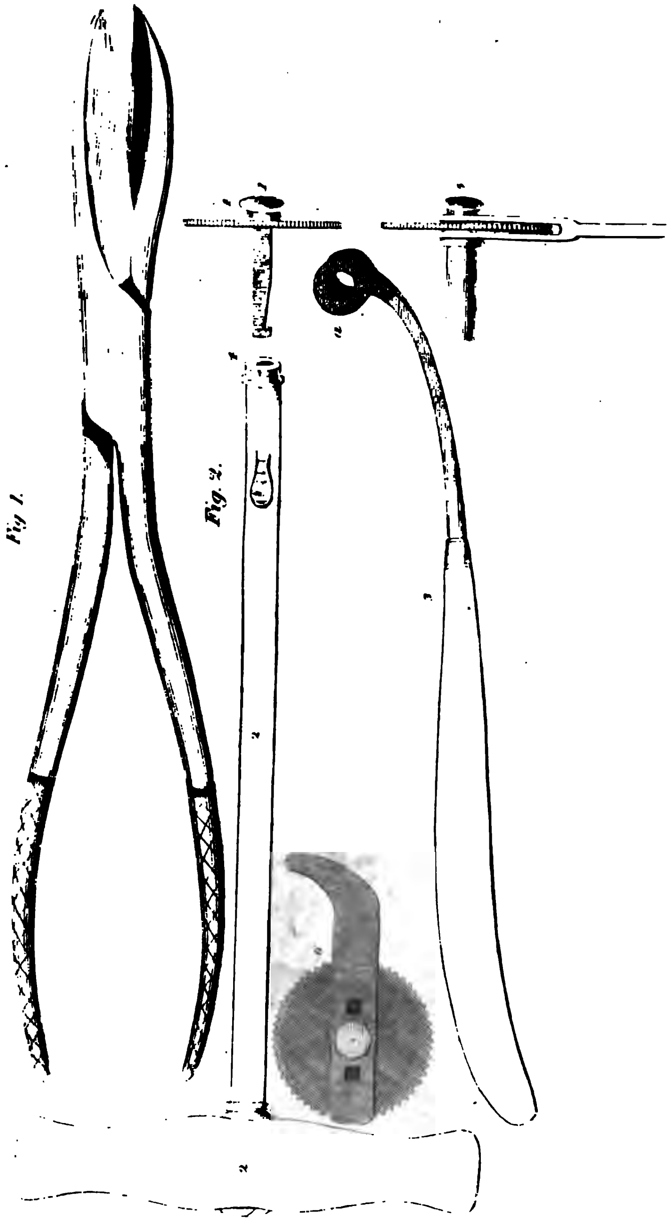


[illegible] Edinburgh, [illegible]

by which the saw is actuated, and having broad tops, are thereby enabled to have upon each of them one or two of the saw-teeth. This edge of the ring is thicker than the inner one, answers the same purpose as the set in surgical saws, and which have lately been much improved by that means. Saws of various dimensions are necessary, but those of the size of an inch, an inch and half, and two inches, are more frequently wanted in most surgical operations; but it must be observed, that although the saws require to be of different diameters, yet the depth of the ring must be the same in all, in order to use every sort of saw in one conveyance. The conveyance then is represented by P P, and into it the part just described is fixed by two small screws at N, which keep the saw firm and secure, in all the various directions that it may be used. By means of these screws, large and small saws can be easily introduced. The joint at L will allow of the conveyance being formed into different angles, which is frequently particularly necessary in applying the annular saw to the convexity of a patient's head, as in cases of fractures of the cranium, the extremities, or other bones of any part of the body; the small screw S is to secure the joint. There is also another alteration that can be made of the position of the saw at F, to enable us to give it any degree of obliquity when wanted in sawing out a wedge-like piece of bone. The saw is actuated by a wheel at C, and it is therefore placed immediately above, in that part of the instrument which is termed the conveyance; every two teeth of this wheel receive into their intermediate spaces the above-mentioned number of the saw-teeth, and the friction which they produce, at the time the annular saw is used, being applied to those parts of the teeth opposite to their points, their sharpness will thereby be preserved from every injury. The wheels R G H C are only to carry the hand of the surgeon clear of the part upon which he is operating, and to combine the saw to the winch, which is the first power. The highest of these, which is termed the leading wheel, as is represented by R, may be made of any dimensions thought proper, in order to accelerate or retard the motion of the saw. At one end of the axis of this wheel is fixed the winch M, which gives sufficient power to force the saw round when it is acting upon the bone. On the other end of this axis (*i. e.* opposite to the winch at *a*) there is an endless screw that turns the pinion B, so that, at the time of using the saw, it moves slowly round, and is attached to a pair of forceps by a long screw at *f*. These forceps are represented by O O, and by this screw they are made to slide in an even and gradual manner upward on the conveyance, which is of infinite service in regulating and preventing the pressure of the saw upon any loose portion of bone that requires to be sawed in two or three pieces, before it can be extracted, especially where the aperture previously made through the muscles and integuments may be of small dimensions. V is a screw of the forceps, that fixes them firm upon any cylindrical or loose fragment of bone that wants dividing; by the pinion turning round, it is brought in an easy and steady manner into contact with the teeth of the saw, and can be at once sawed off with the greatest accuracy and ease. When the sawing of the bone is finished, the ends of the forceps will then be at the upper edge of the saw; and in beginning again to operate, these ends must be let down a little below its under edge; and in order to do this without turning the winch the contrary way to that it was before turned when in the act of sawing, there is a loose piece screwed down upon the fixture *g*, which secures it at the time of sawing This piece is represented by Z, and when the alteration of the forceps just mentioned is to be made, it must be unscrewed as far upwards as it can be brought, and *then* the pinion must be pressed down below the endless screw, and if this piece is still further turned, and the pinion being clear of the screw, the forceps will then go gradually down until their points are sufficiently below the saw edge, that they may be again fixed on another piece of bone; after this the pinion must be drawn up close under the fixture *g*, and the piece Z being screwed tight down, it is then prepared for another operation. At the end of the forceps I have added two small points, which are found very useful in applying the saw to the cranium. These were suggested to me by my friend Mr Ingham, a most eminent surgeon of Newcastle. Q Q are other two points previously made for the same purpose of using it upon the cranium, for preventing the saw, which would otherwise, in a small degree, be propelled forward by the winch, from deviating in the least from any part during an operation; and as these points may be applied to the surface of a piece of board, or upon an operating table, it will be found necessary to press rather forcibly upon them, as the saw will be secured in this manner in every position, and is thereby kept quite firm and steady. From the foregoing description, it is presumed that the advantages of this saw, in surgical operations, will be evident. Thus the annular saw and common scalpel will in fact supersede all the numerous variety of other instruments in every case of surgery, where removing a portion of bone is the object of the operator.

N. B.—Having thus, as I at present conceive, brought my annular saw to perfection, it is but justice to acknowledge, that

Mr Liston's Circular Saw & Bone nippers



K. Mitchell sculp.

Published by A. Constable & Co. Edinburgh 1st July 1823

I have been greatly indebted to the Earl of Stanhope for several suggestions toward the completion of that object.

Wolsingham, October 1, 1814.

II.

Account of a Circular Saw, and of a new form of Bone Nippers.

By CHARLES GRIFFITH, Senior Surgeon to the Forces. Communicated by Dr R. HALL, Detached Military Hospital, Chelsea.

THE military practice of surgery embracing a greater variety of casualty than any other, owing to the fortuitous passage of balls through, or their lodgment in, every part of the body, urging military surgeons occasionally to the exertion of their inventive faculties, for the purposes of preserving life, relieving pain, or avoiding, as much as possible, a too sanguinary use of the knife, it becomes, I think, a duty which they owe to their profession in general, to make public such matters as they have reason to believe may prove of public utility. Influenced by this persuasion, I am induced to offer a mite to your valuable treasury, in drawings of two instruments, one of which in particular, the circular saw, has been much honoured by the approbation of such respectable professional characters here as it has been shewn to. Of the other trifle I have only to say, that it has been particularly useful in my practice, enabling me, by nipping into two or more portions long and deep-seated fragments of bone, and thus to extract the same piecemeal with the common straight or curved forceps, sometimes without dilating the sinus, but always with but little incision, in comparison with what otherwise would have been necessary for such extraction.

These instruments were, indeed, made for specific purposes originally; but the saw, by the help of various sized plates, from the circumference of a sixpenny piece to that exceeding a dollar, being applicable to a variety of purposes, may often come in aid of the civil as well as military surgeon.

The instrument called Hey's saw requiring much pressure and force, fails us when applied to small moveable bones; for instance, the metacarpal or metatarsal; and, however eligible it may be to trace out our own line in the removal of any part of the cranium, for which purpose this instrument appeared to have been calculated, the weight or pressure necessary to form the groove with it becomes not a little hazardous, where inequalities or pits exist in these bones, and highly unpleasant to use, when the perforation is nearly completed. Of the saw here recommended, whilst it is equally capable of forming what curvature we please, the most powerful man can have no apprehension, a heavy pressure of the left hand, holding the support, rather serving to counteract and impede, than to facilitate the quick traversing of the saw in the right.

My instrument has no such guard, as is shewn in the reference marked. 6, it having been desirable to preserve the semidiameter of the saw entirely effective. Some of my friends, however, have judged that this might prove necessary occasionally to prevent the laceration of surrounding soft parts; and this may be done as is here shewn, or by a separate bit affixed to the centre.

For my graphic attempts, defective as they may be, I have only the apology, that I should have been still more deficient had I attempted a delineation of these instruments by words. Should they be to the purpose, however, of your very useful and deservedly popular work, a little of the engraver's aid will, I trust, render them intelligible.

Devonshire Street, Queen Square.

III.

Observations on the Fever prevalent in the province of Guzzerat, with General Remarks on the action of Mercury in the Diseases of India. By A. Gibson, Bombay Medical Department.

It is now pretty generally known, that, in the fevers of India, mercury alone is to be relied on in the early treatment, to obviate immediate danger.

It may be supposed to have three modes of action: *1st*, On the hepatic system; *2dly*, On the intestinal canal; *3dly*, On the general constitution.

Probably all these modes of action are essential to a perfect cure; and if either is deficient, the certain consequence is death, or chronic obstructions, which only yield, if ever, to a change of climate.

1st, If the liver is not acted on, it must, from the determination of blood to it, during the increased febrile action, be in great danger of being disorganized, or of its penicilli becoming consolidated, as a termination of the inflammatory state.

2dly, If the bowels are torpid and constipated, the liver will still be in the same danger; for though it may be pervious and active enough to eliminate bile from the blood sent to it in the healthy state, and in the moderate action of the system, yet during the continued accessions of fever, it may be overpowered by the increased sanguineous afflux, which must either augment, or continue stationary, as long as the alimentary canal refuses to be moved by such means, as would reduce or abate the volume of circulating fluid.

3dly, I have commonly observed the cure to be incomplete, unless the general constitution was affected; for such is the type which the fever very frequently assumes, that, unless counteraction is excited in the system, by the specific power of mercury, the healthy state both of the liver and bowels is inadequate to a cure; the paroxysms become continued; the febrile state is established, and in progress of time irremediable debility follows.

The species of fever, which I have seen prevailing in the province of Guzzerat, partakes chiefly of the typhoid character, though commonly denominated, I presume incorrectly, bilious. It differs from the latter form of fever in requiring less evacuation; and from the former, in the remission being such as to admit of stimuli being administered. The effects of stimuli are what one would look for in an inflammatory diathesis; yet excessive evacuations of any kind seem only to hasten the fatal termination.

In reasoning on this fever, which is the most perplexing and vexatious that a medical practitioner meets with in India, from the difficulty there often is in introducing mercury into the system, I have been led to conclude, in a majority of cases, *First*, That a chronic inflammatory state of the lymphatic, glandular, and secreting systems, has been gradually excited by a slow febrile action, thereby preventing the transmission of the mercury into the system at large; and, *secondly*, That unless the mercury was got to act so as to dissolve this state, if I may be allowed the expression, *in salivation*, a criterion of these systems being free and unobstructed, the fever might, for the time, accidentally be subdued, but the patient will be left a prey to lingering chronic obstructions, till death, long wished for, at length puts a period to ten thousand indescribable sufferings.

This hypothesis of inflammatory lymphatic glandular action existing, however slow it may have been in its progress, seems more consonant to reason, and is more easily reconciled with the febrile state of the general system, under a tropical sun, than torpor, which has been so universally and undefinedly applied. Torpor presupposes a want of stimuli; but any other stimulus than mercury given at the commencement of the fever, and till mercury acts to the full extent on the system, and exerts its own peculiar specific power, would only bring about the sooner what we wish to avert. Buboes in the groin and arm-pits, either preceding or consequent to constitutional fever, cannot surely have their origin in torpor. Yet in the very violent fevers of our own island, such cases are not wanting to every practitioner of much experience.

But, in virulence and phenomena, perhaps the plague bears a closer analogy to the fever in question, and affords a more convincing illustration.

The proofs which have convinced me of the glandular and secreting systems being peculiarly affected, and these independent of hepatic derangement, are,

1st, The dry clammy state of the tongue, mouth, and fauces, and incessant and insatiable thirst, which often long precede, and always follow the subjugation, but imperfect cure of the fever.

2d, The morbid state of all the secretions, and offensiveness and putridity of that of the gums and fauces especially, even before mercury has been given.

3d, The loss of appetite and depraved taste.

4th, The unnatural evacuations, often consisting alone of muco-gelatinous matter, fetid and ill-coloured.

5th, The constant dry, parched, and hot skin.

6th, The emaciation.

7th, The sound state of the liver on dissection.

The affinity between the constitutional symptoms at the period either preceding the attack of fever, when the patient has been long languishing and unwell, or consequent to it, when the mercury has acted imperfectly, and hectic fever, cannot but strike every observing practitioner. Irregular accessions of slight rigours, sometimes quotidian, and sometimes not recurring for days, at uncertain intervals; *burning heat of the palms and feet, extending up the legs;* the feelings, and actual heat of the body, always above natural; a quick pulse, readily increased by the most gentle exercises; the easy excitement of the system to high febrile irritation, by the smallest meal of animal food and use of wine; the flushed countenance; cold clammy sweatings at one period, and dry, hot, parched skin at another, with emaciation, seem to correspond with the phenomena of hectic. But as the phenomena in question occur without suppuration, we must seek for a cause in the general debilitated state of the system, unless an idiopathic origin is allowed; and although I am not prepared to defend an opinion on this important point, the farther investigation of the subject by others, may substantiate the hint at some future period. A change to a cold climate, if timely adopted, or even to another with fewer natural disadvantages, and if by sea, so much the better, fortunately, in most instances, serves towards a recovery. In the pining state above described, are the majority of those composing the convalescent-list of an European regiment at sickly stations. Among the officers also who embark for England on sick-leave, will be found a very large proportion in a similar state. But the soldier, from his humble situation, has not this resource at command, but must patiently wait till a relief of his regiment takes place, when the only chance of a recovery is in his power; but in this hope how many perish! medicine being now exhausted on him in vain.

Absolute confinement during this unhealthy state of the body, is not often long resisted, the person going about his usual occupation, unwilling to lay himself up in a country where the depressing passions are so predominant, and disease so fatal. But, with a multiplicity of uneasy feelings, and a gradual decay of constitution, yet ignorant where to assign his chief complaint, in sleepless nights and restless days, he lingers on a life of extreme misery, till debility, or fever, or its relapse, compel him to his sick-chamber.

I presume to borrow a proof also of the glandular system being peculiarly affected, from what is too often met with in highly inflammatory dysentery, when the constitution cannot be impregnated with mercury, which probably can be explained on no other principle than that of an obstructed abdominal glandular system; for although the disease is in the class Profluvia, the drain from the general system is by no means so great as to carry off the effects of a medicine so assiduously applied as I have generally seen the mercury in all its forms in India, did it enter the constitution.

I have commonly seen the same happen in enteritis, when conjoined with phlebotomy, and other means of depletion,—the mercury has been pushed to an incredible extent, without any sensible or perceptible effect.

In better climates, the phlogistic state of the system is adverse to the introduction of mercury; but the prudent abstraction of blood happily reduces it to that standard which is most favourable for its action. In India, however, in fever, the disease in which this is most speedily to be desired, the same mean would but in very few cases be admissible; for the debility is so great and instantaneous, as well as the tendency to putridity, that only in the robust new-comer is it, if ever, to be hazarded. *

I have only seen it used beneficially, where local pain indicated inflammation to be going on in the contiguous viscus. This, however, is foreign to the fever which I am describing; for, most commonly, no uneasiness is complained of, but the general feelings of pyrexia, and the low delirium and stupor so soon follow, with the sinking pulse, that no further information is to be accurately obtained from the patient; and dissection generally demonstrates nothing more than the congestion in the brain, usually met with in the fatal cases of typhus.

In this low state of the system, no preparatory steps are required by evacuation, further than the care and attention to the unloaded and free state of the stomach and bowels, so necessary in all fevers. On the contrary, in many instances, so great is the debility, that an early tonic is indicated; for it would seem that debility, as well as a plethoric system, are equally inimical to the specific mercurial action. And if the patient is fortunately invigorated sufficiently in this way to give the mercury influence, and before any organ essential to life is injured, by the strictest nursing and attention afterwards, the recovery is almost certain, all morbid action yielding from the moment ptyalism is brought on. But often during this long low period, when every effort is making to mercurialize, the quantity introduced, but as yet inactive, is so great, that when the effect is accomplished, such is the profusion of the ptyalism, that the most disagreeable consequences succeed, and a long and precarious period of convalescence. It is therefore a desideratum, the greatest in the treatment of this fever, to know a criterion by which to judge that you have pushed the mercury to the necessary extent, and no farther. In one instance, where the patient was fast sinking and harassed with excessive diarrhœa, after long mercurial inunction, and the very large exhibition of calomel, in commiseration of the last moments of one apparently moribund, all further medicine was desisted from, but such as would give temporary vigour under causes so debilitating, while the skin was yet hot

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* The spontaneous hæmorrhages which are so distressing in the worst cases, from the nose, mouth and ears, have always appeared to me to hasten death. Indeed, I do not remember an instance of hæmorrhage which did not prove fatal, and without exhibiting the smallest remission, not even before the period when it might with certainty be considered an untoward, and a truly alarming occurrence.

and parched, tongue black and dry, thirst insatiable, and pulse rapid. The effects were marvellous. In twenty-four hours after, the gums were inflamed, and in forty-eight the salivation was begun, and with it all symptoms of previous disease vanished. This, I beg it to be observed, was accidental; and, since the same cause did not once occur again, during a long period, among the sick in a large and crowded hospital of one of His Majesty's regiments, it may be inferred that a criterion cannot be derived from it. This case, however, afforded a clear illustration of the inactivity of mercury in certain states of the system, and also a useful caution against persevering beyond a certain extent in its use. In some other cases, the greatest debility only demanded similar treatment; and although many of them had an equally happy termination, it is a period often too late, and always too precarious to wait for, did any symptoms point out another at an earlier stage, even if only of probable issue.

No inquiry can be attended with a more beneficial result, if successful, than that which is now pointed out; for so universal is calomel in use, and so sovereign is it in efficacy, above all medicines yet introduced into Indian practice, that, unless administered by rule, and watched strictly in its operation, there is much dread of its getting into undeserved disrepute. Those of my professional friends in India, who, with myself, have lamented, in so many instances, the futility of medical science in climates so deleterious, will, I trust, before the conclusion of their valuable services, by their researches into the arcana of disease, yet throw light on a subject so very obscure as the diseases of India still are. If, after the system is already saturated with mercury, and in a disease too of the greatest debility and tendency to putrescence, a medicine so very powerful as calomel be persisted in longer, in the vain expectation of effects which will never become apparent, it is not being too rash, perhaps, to pronounce every grain given above a certain quantity to be prejudicial, and when increased to a greater extent, an active poison.

It may seem empirical to European practitioners, that calomel should be given, apparently so indiscriminately, in the diseases of India; but in all, either a counter-action to that existing in the system at the time, is supposed to demand its use, or it is rather to be presumed, perhaps, that the inflammation prevailing in many of them is of a peculiar and specific nature, as modified by climate, and will only yield to it. In fevers continued or remittent, and in dysentery and diseased liver, acute or chronic, it may be considered a palladium in medicine; but in the unmixed enteritis, which is too often insidious in its approach, and beyond the skill of the physician when first complained of, it is of very doubtful virtue. The preparations of mercury to be relied on are only the submuriate and the ointment. The blue pill is perfectly inadequate to any good purpose, and generally quite inert in India. It becomes a drug of no small danger by being trusted to where the most active are wanted; and by its *only* exciting to inflammatory action, instead of producing a speedy ptyalism, which alone can be of benefit, it thereby increases, instead of diminishes, a diseased state of the system already existing. It may not, perhaps, be deemed misplaced here, to notice a practice too much in use, and too pernicious in its effects, not to merit a brief cautionary observation. I allude to the frequent small doses of calomel taken by so many of the residents in warm climates, as prophylactic of the diseases peculiar to these regions. By the very medicine which would be resorted to as the sheet-anchor in the most urgent case of disease, that state is caused which is so much to be obviated; for, by keeping the system constantly excited, in a small degree, glandular obstructions, with all their concomitants, are certainly produced, and the foundation is laid for perpetual ill health, and ultimately a broken constitution. To such as favour this essay with their perusal, it may meet their wishes to be informed of the tonic given in that stage of fever at which mercury was left off. A mineral acid, but, above all, the nitric, is that which can with safety be ventured on, and it will be found to disappoint less than any other medicine. The cinchona, and all the class of bitters, only load the stomach, and increase the febrile irritation. Nitric acid is tonic without over-stimulating. It is a grateful and cooling beverage to the parched mouth and burning body; it is therefore febrifuge; it is antiseptic, and in these combines the good qualities chiefly wanted at this period. The best test, perhaps, of its pleasant virtues, is the incessant call made by the sickly patient for the acid drink he got when last in hospital.

IV.

Observations on the Nature of the Climate, and the Fevers which prevail at Seringapatam. By A. Nicoll, M. D. Member of the Royal Physical Society of Edinburgh, &c.

Ever since the British took possession of Seringapatam, their forces, both European and native, have greatly suffered from the insalubrity of its climate. Any investigation, therefore, into the nature of the climate, and diseases which prevail there, becomes peculiarly interesting and important.

Various *Reports* concerning the nature of the diseases which appear amongst the troops at Seringapatam, have been drawn up, by very able and intelligent medical officers, for the information of Government; but it is much to be regretted, that none of these have ever appeared in print, or have been generally diffused for the guidance and instruction of the medical officers of this presidency.

It appears that these statements have contemplated the nature of the climate and diseases which are peculiar to the place, under different points of view; some, considering the troops as being as healthy at all seasons there, comparatively speaking, as at other military stations of the presidency. Others, on the other hand, have declared, that the sickness which occurs is, at all seasons, much greater, and the number of deaths in a given period more numerous. The principle of calculating the salubrity of military stations or posts, by the number of deaths which occur in a certain proportion of men, in a given period, is generally the criterion by which an estimation is formed. This principle, though perfectly applicable to the population of a country or town, becomes erroneous when applied to a moveable population, such as a regiment. The quantity of sickness will give us the efficiency or inefficiency of the corps, and the healthiness or unhealthiness of the station, while that by deaths will not. Two different corps at two different stations, and of a similar numerical strength, may lose an equal proportion of men, yet the one may have had double or more sick in the period, than the other. This, I think, has been too little attended to in making statements of the healthiness or unhealthiness of corps and stations; and has tended, in no small degree, to mislead the minds of those who are entrusted with the high powers of government.

The following observations made on the nature of the climate, and the fevers which appeared amongst 700 Europeans and some native corps stationed at Seringapatam for eighteen months, will, I hope, place this subject in a more clear and satisfactory light.

Intermittent fevers are prevalent in every part of the Mysoor country, but are much more common at Seringapatam than in any other; and they vary according to the changes of the season and conditions of the atmosphere. In the hot months of the year, the fever becomes remittent or typhoid; the latter usually of that species denominated by Cullen *Typhus icterodes.** As the season cools, and the weather becomes more steady and pleasant, the remissions of the fever become more distinct; and as the weather gets what may be called cold, the regular *agues* are formed. Dysentery is frequently combined both with remittent and intermittent fevers; but is more common in the cold season than in any other. There is nothing peculiar in the approach of the remittent, much less in the ague. The yellow-fever always presented itself in the beginning like a severe remittent, generally with great sickness at stomach, and vomiting of a greenish or bilious matter. A flushing of the face, and a degree of stupor and listlessness; a burning skin; full and quick pulse; frequent respirations, and excruciating pain in the head and loins, were the great pathognomonic symptoms of the disease. When at this stage of the disease a stop was not made to its further progress, still greater excitement and irritability of the functions of life came on, and incessant vomiting of a greenish or yellowish-coloured matter, delirium ferox, and sometimes dysentery, with great violence succeeded, and, in the course of a few hours, put an end to the sufferings of the patient. On or about the third day of the disease, the yellowness of the body generally appeared; the adnatæ, the neck, breast, and belly, showed at first the partial transfusion, which became deeper in colour, the higher in violence the disease arose. Though the disease run its fatal course in a few instances in forty-eight hours, yet it was generally on the sixth or seventh day that they died. This so often happened, that whenever I got my fever patients over these two critical days, I contemplated a speedy solution of the disease at hand.

The appearance of this form of fever, so similar in its progress and termination to that which prevails in other tropical countries, renders it a truly interesting and important subject of inquiry, to ascertain upon what particular circumstances and peculiar constitutions of the atmosphere its origin seems dependent.

I. From the local situation of the island of Seringapatam, its climate is subject to sudden and great vicissitudes of temperature. Its site is low and moist, surrounded on all sides by a high ridge of mountains, which exclude it greatly from receiving the beneficial influence of the Coromandel and Malabar monsoons. The rains are but partial at all seasons, except in the beginning of the monsoons. The Cauvery river, which, by its branching into two divisions of a considerable size, and by their reuniting again, forms the island of Seringapatam, never begins to fill until about the latter end of May, or beginning of June, when the Malabar monsoon has commenced; at which times the husbandmen begin to cultivate their paddy fields, and sow their grain. These fields, for many miles around the island, are the greater part of the year under water; some containing vegetating grain, while others hold the roots and stalks of a former crop in a state of putrefaction. There are also large tracks of paddy fields, which undergo similar processes, on the island itself.

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* Synopsis Nosolg. Method. cl. 1. Pyrexiæ, Ord. I. Feb. Gen. V. Typhus Sp. II.

II. The four first months of the year are excessively hot, close, and sultry, until the Malabar monsoon sets in, in May. At 5 in the morning the thermometer is generally about 65°, and, at 3 in the afternoon about 94° Fahrenheit. In May and June, by the refreshing showers and breezes wafted from the mountains, which separate the Mysoor from the Malabar country, the climate is rendered tolerably healthy and pleasant. Again it becomes hot and sultry in July, August, and September, but nothing like to the four first months of the year, until the Coromandel monsoon begins, in October, which, by its mild and salubrious influence, soon effects great and remarkable changes in the air and temperature of the place. At this season, especially in November, the thermometer at 5, P. M. has been so low as 48°, and, in the middle of the same day, has risen to 88°. I have also frequently observed a difference of 40 degrees between six o'clock in the morning, and twelve of the day. During the hot months of the year, the winds are generally southerly or easterly; in the cold season, they become westerly or northerly.

III. The *fort*, in which the troops chiefly reside, is in a very low situation, with lofty walls surrounding it, which, in a great measure, prevent the free circulation of air. Besides the barracks, hospitals, &c. for the forces being bad, and highly objectionable, there is an extensive buzar close to them, which, by its filth and situation, becomes no small nuisance to the Europeans.

From these local causes, which can never completely be removed, is the climate of Seringapatam rendered unhealthy. It is true some of them seemingly might be removed, or at least moderated; others must remain as durable as laws of nature. While, therefore, the cause is preserved, the effect must be permanent.

I. Other sources of noxious exhalations are abundantly fruitful at Seringapatam. These, together with a moist sultry atmosphere, subject to great changes of temperature, from intense heat to extreme cold, have, in all ages, been viewed as the origin of pestilence and death.* In the ditches between the ramparts, and in various parts of the fort, where all the Europeans, and many thousand natives reside, are constantly deposited all the filth and corruption of the place. On the banks of the *Cauvery river*, and in several places of the island, pools, stagnant with offensive and putrid matter, are to be seen. All the mass of animal and vegetable corruption from a population, including Europeans and natives, no less than 90,000, is collected on a small space of ground, the circumference of the island not exceeding three miles. These materials of putrefaction, for about eight months of the year, lie in those repositories which I have mentioned, until the periodical rains of Malabar begin, which, falling in the *ghauts*, run down, and fill the Cauvery river. The filling of this river is always very sudden, and it comes rushing along with great impetuosity; sweeps out all the filth from the ditches; clears away all the impurities, so long stagnant in the island; and leaves the place, for a while, tolerably healthy, and the air cool and refreshing.

II. This change of the season does not fail to produce a corresponding change in the baneful fevers of the hot months. This has long been observed by medical writers,† and is verified at every return of season there. The noxious exhalations floating in the atmosphere, and possessed of a higher degree of virulence during the hot weather, must be considered as the primary and essential cause of the disease. The degree of heat which seems indispensable to concentrate the power of this poison, sufficiently to produce the disease, is higher than 80° of Fahrenheit's thermometer. While the thermometer was about 90°, there prevailed the greatest sickness and mortality; when the air cooled, and the thermometer came below 80°, the yellow-fever was not to be seen,—but more cases of common typhus and intermittent fevers occurred. Thus it appears, that the yellow-fever never exists but in higher degrees of heat than what is necessary for the support of the virulence of common typhus fever or the plague; and that the *nidus* of each requires a nice concurrence of certain degrees of heat, moisture, and nutriment to animate them.‡

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[*] Hippocrat. Op. om. De Epid. Lib. I. c. iii. p. 238.

[†] Mead de Pestis, Part I. c. ii. p. 191. Sir John Pringle, Diseases of the Army, Part II. chap. ii. p. 118.

[‡] Vide that ingenious letter of the celebrated Sir G. Blane to Baron Jacobi Klöest, in Edinburgh Med. and Surg. Journal, Vol. III. No. 12. p. 385.

For these reasons, and others, (which the limits of this paper, already, I am afraid, too long, will not allow me to relate) I am fully convinced, that the fevers which prevail at Seringapatam are generated in the place; and that their seeming varieties are altogether dependent on the same remote cause, modified by the operation of changes in the sensible qualities of the atmosphere. The same cause, therefore, which, in the cold season, is only calculated, by its powers, loosely combined, to produce *agues* or *remittents,* acquires, from the rays of a scorching sun, under a concurrence of favourable circumstances, a wonderful degree of concentration in its elements, and seemingly the nearer they approach to each other, the higher their pestilential agency becomes.*

With regard to the infectious nature of the yellow-fever, some doubts are entertained, from never observing a single orderly attending those ill with the disease, or any of the other patients in hospital, who were oftentimes indiscriminately mixed together, for the want of room to put our sick and convalescents in, contracting the disease. However, the prevalence of this disease being regulated in its operation by a determined range of atmospheric heat, and, from numerous facts related, especially by that enlightened physician, Sir Gilbert Blane,† I have no doubts but that, under certain circumstances, in regard to the constitution of the atmosphere, and the susceptibility of individuals, it may evince an infectious nature. It is certain, that *marsh* and *animal* effluvia are never found to act but when they are near to the sources from whence they issue; and it is probable, that the difference of their effects may always be traced to these sources, as modified by internal causes, and peculiar constitutions of the persons affected. We are still ignorant of the chemical nature and exact sources of human and marsh effluvia; but it appears they are sometimes intimately combined, and, according to the state of combination in which they exist, the different varieties seemingly of contagious fever are produced. The various remote causes of fever have not been well ascertained, but the genera and species of fevers have been pretty clearly pointed out; and in each of those species, it is probable, the effluvia are of one specific nature, though arising from one common source, but acquiring their peculiar characters from certain states of the atmosphere. The yellow-fever may, therefore, only arise from a combination of *animal* and *marsh* effluvia, operating under circumstances, as above pointed out.

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* Galen de Temperament. Lib. I. c. iv. and Comment. in Epidem. Lib. III.

† Blane, Diseases of Seamen, p. 605.

The persons who were most subject to yellow-fever at Seringapatam, were the strong and robust, who had exposed themselves carelessly to the vicissitudes of the climate, and lived irregularly. Those who had been much exhausted by almost habitual drunkenness, and long residence in India, were the first who suffered, and fell victims to the disease. Three instances came under my notice, where, in characters corresponding to the above-mentioned, the powers of life were destroyed in the first paroxysm of fever. Irregularity, drunkenness, and exposure to the changes of the climate, when the body is in a state of perspiration, or *indirect debility*, are powerful agents in rendering the functions of life susceptible of morbid associations, or liable to the impressions of the morbid *virus*, yet certain situations in respect to dryness and ventilation, though equally exposed to noxious blasts or exhalations, make no small change in the prevalence and nature of fever. I observed, that out of 396 Europeans, who lived in barracks on the *ground-floor*, there were 956 cases of *fever* in eighteen months, of which 25 died. And out of 294 who lived in the *upper floor* of the same barracks, in the same space of time, only 552 were taken ill with fever, of whom 13 died.* I had occasion to remark, too, that while those *companies* who lived on the *ground-floor* were always more sickly than those in the *upper one*; that again they varied in themselves, in regard to sickness, in proportion as their barrack-rooms was exposed to the easterly or westerly winds.† The same remark might be made in regard to situation and exposure of the gentlemen's houses on the island, and in the fort more particularly.

Appearances on Dissection.

The anatomical examination of the bodies of those who died of the yellow fever, were made with considerable attention and minuteness; but the appearances of the morbid structure of the most important organs, those connected with the functions of life, and seemingly with the disease, were by no means uniform or satisfactory, nor could they in any instance be applied to the full explanation of the morbid actions, which appeared in the rise, progress, and termination of the case.

Brain,—Always contained in its ventricles a large proportion of serum, and its vessels were generally turgid with watery blood.

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* Sir John Pringle, Diseases of the Army, Part I. chap. ii. p. 15. Part II. chap. iii. p. 96.

† Lancisi, De Noxiis Paludum Effluviis, Lib. II. Epid. I. chap. iii.

Chest,—Seldom shewed much signs of morbid alteration in any of its viscera. Sometimes the *heart* appeared enlarged, and the *pericardium* contained more water than natural. At times larger portions of lymph, or polypi were found in the venæ cavæ, right auricle and left ventricle. The blood was always very dark, and watery, running soon into putrefaction.

Abdomen,—Presented various morbid appearances, slight marks of inflammation on the pyloric portion of the *stomach*, but apparently proceeding from the acrid matters found in it, as the duodenum, which contained nearly similar matters, presented the same appearance. The *intestines* always held large quantities of fetid matter of various colours. The *liver* was rarely found anywise diseased, but there was always a large secretion of bile. The *gall-bladder* was always turgid; frequently large quantities of bile were seen floating on the surface of the intestines. When the bodies were inspected a few hours after death, the bile was *yellow*, but when kept more than twelve hours, it became black and putrid. The bile was always very bitter, and of rather soapy consistence, running speedily into putrefaction on exposure to the air. It changed the vegetable blues to a green, which, on standing for about three hours, turned red, and sediment was deposited. On evaporating bile occasionally, by a gentle heat, the watery part appeared inconsiderable, in proportion to the residual matter, which was in immense quantity, of a darkish colour, soluble in water, but readily going into the putrefactive process. When alcohol was added to the bile, there was very little albumen coagulated; the dark greenish-coloured fluid which remained, afforded by evaporation always a larger quantity of *resin*, than what I could ever obtain in a similar proportion of bile, taken from those who died of dysentery, or were killed by accident. The liquors found in the pericardium and ventricles of the brain, as also in the cavity of the abdomen at times, partook but slightly of some of the properties of bile; they were, however, sufficiently clear, as to put it beyond doubt, that the yellowness of the skin, and fluids of the body, in yellow-fever, proceeds from the bile having entered into the circulation, and communicated to them its colour.*

From these facts and observations, I am sorry to say, I cannot derive that advantage and important results to the practice of medicine which might be wished. This branch of medical science, which has for its object the ascertaining the seat and causes of diseases in organic derangements, affords ample fields for the investigation of physicians and anatomists, and can only be perfected by their unwearied exertions.[*]

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* Blane, Observations on Fevers, Part III. chap. I. p. 411.

The plan which was found most successful in curing the yellow fever at Seringapatam, was that which formed its indications: on, *1st*, removing the violence of reaction, and, *2dly*, preventing exhaustion of the system by a recurrence of the fever. When the violence of reaction and inflammatory diathesis were sufficiently manifest, blood-letting was employed, the quantity extracted being regulated by the strength, age, and plethoric state of the patient. The appearance of the blood, when drawn, was no criterion whatever. In no instance, where general bleeding was had timely recourse to, and the quantity judiciously taken away, did the reaction of the system, the morbid heat, and general irritability of the animal and natural functions, continue unabated in violence. When the disease has just commenced, in any constitution, whether robust or plethoric, or weak and emaciated, if there are symptoms of any inflammatory diathesis, bleeding must be employed.[†] Small doses of calomel and neutral salts must be exhibited every hour, until the bowels are unloaded of their morbid contents, and the capillaries of the skin opened, and the surface becomes moist. But, along with the exhibition of those medicines, and after bleeding, while the skin is dry, the respirations frequent, and the animal heat 103° or 108°, the cold affusion must be resolutely and judiciously applied, and repeated, until the reaction of the system, and progress of the disease, are arrested. The cold affusion is the most powerful remedy in subduing the fever; and the only preventive against the irritability of the stomach, was keeping the bowels open by small doses of calomel and jalap, or solutions of the neutral salts. As soon as a distinct remission was obtained, it was found absolutely necessary to throw in the bark and wine, and prescribe a very nourishing diet, in order to prevent a recurrence of the fever, which, though subdued, is apt to return again and again as before. I found the bark thrown up by injection into the rectum, a valuable remedy in cases where the stomach was irritable and nauseated it. In intermittent fevers, I have often exhibited it in the quantity of an ounce, joined with a little tincture of opium, in this way, just before the expected return of the fit, and in no instance did it fail of moderating the violence of the fit, if it did not succeed in preventing its return altogether. *

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[*] Cabanis, Revolutions of Medical Science, translation by A. Henderson, M.D. p. 294.

[†] Jackson's Treatise on Diseases of Jamaica, p. 51.

When there was great irritability of the stomach, constant vomiting of greenish-coloured matter, great morbid heat of the skin, delirium, and much exhaustion of the powers of life, the cold affusion, constantly repeated, while the spasmodic constriction of the vessels of skin continued, and the morbid associations remained, is the remedy to be depended on; for, while it subdues the principle of fever, it invigorates the powers of life, and enables us to clear the stomach and intestines by gentle cathartics and laxative glysters. These remedies, when judiciously applied in the early stages of fever, will seldom fail indeed to stop its progress, or bring it to a speedier issue; but they are not effectual in preventing its return where the body is again exposed to the cause which first produced it. Bark is the only remedy to be depended on, and when there is any morbid derangement in the *liver* or *spleen*, mercury must be employed. Blisters applied to the *head* and *stomach* were often of great service. When the paroxysm was subsiding, small doses of *opium* and *æther* were given with the most salutary effects. Under the above system of treatment, when the patient was brought to us on the first or second day of the disease, we generally succeeded in producing a final solution of the disease before the fourth or sixth day. When the fever continued beyond this period there was always great difficulty in putting a stop to its progress, if it did not kill the patient then. If the bowels were not kept open, and every slight exacerbation of fever checked by the cold affusion, the disease generally terminated fatally, sooner or later. But when any slight accession or exacerbation of fever was carefully watched and stopped by the cold affusion, applied in one way or another, a considerable remission at last took place, which enabled us to give the bark, and support the powers of life by due stimuli. Carrying the effects of calomel so far as to produce salivation, was never found necessary or beneficial in the beginning of the disease, but often found valuable, in conjunction with the bark, when the disease chanced to vary its type, or continued long, and gave us some reason to suspect the presence of some organic derangement, or dropsical diathesis. It thus appears, that the treatment of fever, of whatever kind or form, unaccompanied with organic derangement, is, now-a-days, both as simple and successful in India as in Europe.

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* Heberden, Commentarii de Morb. Hist. et Curatione, cap. xxxviii. p. 160.

I beg leave to subjoin an *abstract* of the *diseases*, with the *cures*, *deaths*, and *relieved*, taken from the tables of the sick-registers of His Majesty's 80th Regiment, (consisting of 700 men), during their residence at Seringapatam, from November 1811, to April 1813.

	Numberadmitted into Hospital.	Cured.	Deaths.	Relieved.
Fevers, -	1508	1449	38	21
Fluxes, -	617	580	14	23
Hepatitis, -	76	70	4	2
Phthisis, -	8		4	4
Dropsy, -	27	20	3	4
Tetanus, -	2		2	
Rheumatism, -	184	171		13
Catarrh, -	56	47	1	8
Ophthalmia, -	158	148		10
Variola, -	4	3		1
Measles, -	1	1		
Asthma, -	4	3		1
Wounds and Ulcers,	286	259	1	26
Venereal, -	140	135		5
Grand Total,	3071	2886	67	118

——"Fateor equidem ea esse rudia, inchoata, et manca: cujus rei culpa, ut maximam partem in me recidat, partim tamen in ipsius artis conditionem erit rejicienda."—*Heberden, Comment. de Hist. et Curat. Morb.* p. 416.

V.

Experiment, tending to prove that Gonorrhœa, and a smooth superficial Ulcer, without induration, arise from one and the same Virus. By Simon M'Coy, Surgeon, Dublin.

I beg leave to submit to your notice, the result of an experiment I made upon myself about six months ago, at which time I mentioned it to Mr Carmichael, as an interesting case, tending in the strongest manner to support the doctrine he promulgated, that gonorrhœa, and a smooth superficial ulcer, without induration, arise from one and the same virus. I have delayed the publication of the experiment, as I wished to ascertain if secondary symptoms would occur; and I have the *comfort* to observe that, as yet, nothing of the kind has appeared. I am aware that the experiment has been made before, to prove whether the matter of gonorrhœa can produce a disease incurable without mercury; but the fact being established that it will not, little attention appeared to have been given by the experimentalist to describe the sore produced by this virus. To learn the appearances which such a sore would be likely to put on, I made the following experiment.

On the 14th of August 1814, I procured the infection from a man who had a severe gonorrhœa, and applied it, on the end of a blunt silver probe, to the inside of the urethra, about two or three lines from the orifice. I next infected a lancet, and drew the edge two or three times over the prepuce, near the *corona glandis*. The application drew no blood, and in this state I allowed every thing to remain. On the morning of the 18th, I perceived the discharge had commenced from the urethra, and the part of the prepuce which had been inoculated felt very sore. I laid a little dry lint on it, and suffered the disease to proceed, without taking any medicines, or even using any precautions as to diet, until the 22d, when the discharge from the urethra was completely established, and one sore had formed on the prepuce, elevated in the centre, of a glassy, shining appearance, and of a dusky yellow colour. It appeared not very unlike, in form, the section of a pea, and about this ○ size. Its boundary was accurately marked by a capillary circular line, of a bright red colour. This was made very evident by a magnifying-glass, and the ulcer, through the same medium, appeared as if covered with grains of very fine sand. As my sole motive for infecting the urethra, was to observe if it produced symptoms exactly the same as gonorrhœa taken in the usual manner, and as the point was settled in the affirmative, I entered upon the cure of that part of the disease, by injections of calomel and mucilage of starch. I had felt much inconvenience from *ardor urinæ;* but it began rapidly to diminish, as did the discharge, and in something more than three weeks it was entirely gone. I suffered the ulcer to go on, without any application but dry lint, and I took ten grains of Rufus' pill. This was all the medicines I took until the 29th, when the sore was entirely healed. I thought I was now done with an affair which had given me considerable trouble and uneasiness; but, on the 2d of September, I was astonished in the morning to perceive three sores make their appearance. Two were on the prepuce, near the seat of the former one, and one on the *glans penis ;* the last was rather superficial, but the two former were accurately excavated ulcers, with elevated edges, but without the slightest induration. Their bases were inflamed ; the colour of the ulcer was the same as the primary one, if I may so call it. I allowed them to take their own course until the 8th, when, seeing no material change either in their character or appearance, I began to dress them simply with calamine cerate, and took every morning a pill of four grains of Rufus' mass. The ulcers began to decline rapidly, and by the 24th or 25th they were entirely healed.

I have now detailed, as well as I am able, the circumstances of this experiment. Mr Gregory (one of the pupils of the Lock Hospital) witnessed it in all its stages, and can corroborate my statements. I feel happy in presenting it to the public, as it so decidedly confirms the opinions of the gentleman above alluded to, who has so happily elucidated a disease which before was very imperfectly, if at all, understood.

Dawson Street, Dublin, March 1, 1815.

VI.

Case of obstinate Gleet cured by the use of Sea Water as an Injection. By Edward B. Fletcher, Surgeon.

Conceiving every occurrence in medical and surgical practice, where remedies are employed to advantage in the cure of a disease, in some degree interesting, I am induced to hope the following may be allowed to occupy a page in your valuable collection. Mr ——, in the 18th or 19th year of his age, contracted a gonorrhœa, which, in a short time, was accompanied with every distressing symptom of that complaint, viz. ardor urinæ, &c. &c. These attendants at length subsided, but for upwards of two years he suffered from an obstinate gleet, notwithstanding a variety of remedies used by the advice, and under the inspection of an intelligent practitioner. Being in the army, he was ordered to a station near the coast, and recommended to try the effect of local application of sea-water, and to continue the use of an injection he had been previously ordered. This he had forgot to provide himself with ; and not knowing where to procure it at the moment, and that he might *be doing some-*

thing, he injected some of the sea-water. Finding no unpleasant effects from its use, he was induced to continue, and in a few days observed a very sensible alteration in the quantity of the discharge, which continued gradually to decrease, and in about ten days entirely ceased. This was in the spring of 1809. In a letter which I received from him a few days ago, he informs me, that he has never had the slightest return.

In corroboration of the effect of vaccine virus upon the constitution, (although previously variolated, as noticed in Dr Baron's letter in your Journal, Vol. X. No. 38. p. 261-3, I submit the following occurrence to your notice.

In the spring of 1810, while suffering under a very severe attack of herpes, extending over the whole of the neck and lower jaw, I went to vaccinate a little patient. I had put the armed lancet between my lips, till I made a puncture in the arm with a clean one, but, by a sudden motion of the child, the point of the former was forced in contact with my cheek, in which it made a slight wound. About the third day, I plainly perceived a pustule forming, which went through its regular course. I had a smart symptomatic fever. As the vaccine pustule advanced to maturity, the previous eruptions declined, and in about fourteen days were quite gone. I had suffered from three very severe and obstinate attacks of herpes in less than nine months; but since the period of accidental vaccination, they have not troubled me with another visit. I bear the mark of vaccination upon my left cheek, upon my arm the mark of variola.

Carlisle, Dec. 8, 1814.

VII.

Case of Tænia cured by Spiritus Terebinthinæ. By Robert Hartle, Surgeon to the Forces, Member of the Royal College of Surgeons, London.

Lieutenant J—— of his Majesty's military service, æt. 36, requested my advice on a bowel complaint of long standing, (as he termed it). He informed me that he frequently, and particularly before taking his breakfast, had griping pains of the abdomen, accompanied with nausea, sometimes at-

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to stool, without any effect. He
left him on his taking his first re-
that at all times he felt an uneasy weight
had once or twice seen in his stools a sub-
bit of tape, an inch or more in length. It
red to me that this was a case of tænia. I di-
be sparing of his food and drink that day, and on
to take any supper; that I would call on him at six
following morning with a remedy, enjoining him to
having until I arrived. Accordingly, at my appointed
gave him two ounces of the spiritus terebinthinæ (undilut-
a dose. I remained to note the time of any alteration, or
effects of the turpentine.

Twenty minutes after he had swallowed the draught, he complained of excruciating pain in the lower part of his belly, accompanied with a desire to stool, which I directed him to assist by going to the chair. Soon after he was seated, the pain abated, and he voided a tænia (dead) three yards one inch in length, and nearly a quarter of an inch in breadth.

Prior to this, his appearance was meagre, much resembling a case of chronic dysentery, notwithstanding his appetite was voracious. I saw him ten days after, and was much astonished at the great alteration for the better; I have since understood he has become very corpulent.

Although two ounces of this oil may be considered a large dose, it produced no other inconvenience than so much gin, as my patient only complained of a sensation like that of slight intoxication; nor did it determine to the urinary organs, as I expected;—this, I consider, was owing to its acting as a speedy purgative. I was since informed by an army surgeon, that in a similar case he gave an ounce without success; that he afterwards gave two ounces, and his patient passed the worm in fifteen minutes.

Dominica, 2*d January* 1815.

VIII.

On the Oil of Turpentine in Epilepsy. By DAVID LITHGOW, M. D. Coleraine, Ireland.

WHEN so untractable and violent a disease as epilepsy, which has so long been ranked among the *opprobria medico-*

omes before the medical practitioner, the whole Materia ica is ransacked, without the slightest attention being paid the foundation of medical science, physiology. Theory, in many cases, has been obliged to yield to the surer and more pleasing influence of fact; and a fact once established by the test of experience, soon begins to set the head of the physiologist to work to find out a cause. The action of cinchona on the system, in the cure of intermittents, of mercury in syphilis, of sulphur in psora, &c. severally gave the theorists a vast deal of trouble; and, after all, they were obliged to bestow on them the unmeaning epithet of specific. A new field has lately been opened for discussion, by the introduction of the oil of turpentine in epilepsy, by Dr Percival, a physician of great eminence, who, from his situation in the hospital for incurables, has the best opportunity of making improvements in his profession. The doctor, from every day seeing the practice founded on the various theories of no avail, caught eagerly at the slightest circumstance to afford relief to his suffering patients. Taking advantage of a hint thrown out by Dr Latham in his Treatise on Diabetes, he made trial of the oleum terebinthinæ in epilepsy, with the happiest effects. It is probable that this medicine may not turn out to be altogether a *specific*, yet, as affording relief in a complaint which before baffled the utmost skill of the physician, it is of the greatest importance. The action of this, like that of many other medicines, is unknown. Dr Percival draws a conclusion from his cases in the following words: " The only specific action of that drug was apparently emmenagogue; a hint not undeserving of application in other cases." From the three cases related by himself, and one by Dr Latham, in the 36th Number of this Journal, and also from the following case, which occurred to myself, I was led to form the same opinion.

Sarah Gray, ætatis 23, of a robust habit of body, was subject to attacks of epilepsy for six months. The paroxysms occurred at intervals of a month, during the beginning of her complaint, but latterly she has been much oftener seized, once, and occasionally twice in a week. She is affected during the night, and, in a state of insensibility, is violently convulsed for upwards of ten minutes, the fits leaving her in a tolerably good state of health. Bowels regular; catamenia regular, but in small quantity; knows no cause for her complaints. I ordered her an aloetic purge, after which she began to use the oil of turpentine. The second day she had a slight attack; the menstrual flux was increased in quantity, and she never had a return of her complaint.

From this, and the four cases above alluded to, (all the subjects being females) I was led to agree with Dr Percival in opinion, and to think, that how valuable soever the discovery might be to the female, the male sex were to lose all the benefits arising from it. But, from a further employment of it, as a last resource, in the two following cases, (one of which proved unsuccessful) I changed my opinion, and concluded, that although its power was great as an emmenagogue, it was an unknown effect which produced the ascendancy over the epileptic attack.

James Boothe, ætatis 28, of a short stature, and plethoric habit, has been subject, for these ten years, to attacks of epilepsy of a severe kind. Dates the origin of his complaints from a severe fright, since which time he has had regular attacks every month, and sometimes frequent fits for the course of eight days. For a short period after the cessation of these fits he is in a state of mental derangement; during the paroxysm he is quite insensible. Bowels costive, and occasional headachs. I had first recourse to the use of purgatives; afterwards a number of the more powerful anti-epileptic remedies were tried in vain; when I began (February 15th) to use the oil of turpentine, in the quantity of four drachms, diffused, by syrup, in a pint of peppermint-water, a wine glassful was given four times a-day. A week after he had a return of the paroxysm, but in a slighter degree; at the usual period of return he had a few attacks, so slight that they were scarcely perceptible; since which time he has remained in perfect health. In the course of his recovery he made use of upwards of two pounds of turpentine, without any sensible effect. He sometimes complained of a sense of prickling throughout his whole body, with occasional vertigo. The other case to which I alluded was that of Robert Dunn, aged 50. He has had violent attacks of this complaint for sixteen years, at irregular periods; for the most part once a-week. He is insensible of the attack, and during it, but remains well in the interval. The oil of turpentine in this case was used to a great extent, and for a considerable time the violence of the paroxysms were reduced. They however returned, and continue as usual. The cause was lying asleep before an immense fire.

IX.

Case of temporary Loss of Voice from Hysteria, occurring in a Recruit in the Honourable Company's service, Passenger on board the H. C. Ship Marquis of Wellington. By J. A. D. WATSON, Member of the Royal College of Surgeons, London, and Surgeon of the Ship.

APRIL 8th 1813.—DANIEL LAWLER, aged 36, a strong healthy man, complained in the morning of giddiness and violent headach; in the forenoon he was seized with general convulsions; the muscles of the face were considerably distorted; discharge of frothy matter from the mouth; pulse quick and full. These symptoms continued only for a short time, when he recovered his senses, and complained of no particular uneasiness. At six o'clock in the evening he had a return of all the symptoms, as above, accompanied with alternate laughing and crying. Upon making inquiry respecting his sensations, I was much astonished at not receiving an answer, and, after repeatedly questioning him, I discovered, by a shake of his head, and his pointing to his throat, which had no unnatural appearance, that he was deprived of the power of speech. Pulse 89, and full. A vein was opened in the arm, and twelve ounces of blood taken.

9th April.—Is perfectly sensible; muscles of the throat on both sides seem tense, and affected with spasms, so much so, as to prevent him articulating; cannot, without great difficulty, swallow any solid food. The muscles of the left side of the abdomen feel a little tense. Pulse 86.

Applic. Emp. canth. gutturi.
℞ Spt. æther. sulph. gtts. 40.
Tinct. opii, gtts. xx. f. haust. tertia quaq. hora sumendus.

Vespere.—Much in the same way; blister has risen; took a little sago for dinner; does not seem to feel his throat so uneasy, but signs to his head, which appears to give him the greatest uneasiness. Pulse as in the morning, and full; the muscles of the abdomen still feel a little tense. Detrahatur sanguis ad ℥xij.

Cap. Pulv. jal. gr. xxv.
Sub. hyd. gr. vi. statim.
Applic. Emp. vesicat. capiti.

Vomited the medicine.

Injiciatur enema domest. Lint. sapon. cum op. abd. applic.

From this, and the four
jects being females) I wa
nion, and to think, that
be to the female, the r
ing from it. But, fr
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culating; signs to his throat,
rance; spasm gone; blisters on
pulse 80, and full; had two stools
ub. hyd. gr. x. About 3 o'clock he
ch, and expressed a desire for food
ity and slight headach. Medicine ope
From this time he began to recover his
able for duty by the 20th. Says he was pre
ing by a tightness of his throat, and felt as if
n it. I beg leave to subjoin the following obser
disease at first a good deal resembled epilepsy, had
ion of the throat followed, which gave it the ap
f hysteria. The loss of speech, which continued un
rnoon of the second day, "*the space of forty-five hours*,"
never heard mentioned as a symptom in either disease
man has always been very healthy, is now in his 36th year,
has never had a similar attack. Another case, although
ymptoms were not so violent, occurred also on our passage
Madras. ——— Jones, Company's artillery, was similarly
acked on the 29th April, and recovered his speech next morn
ing. The same treatment was used, and he experienced similar
sensations in his throat to Lawler. Dr Trotter, although he does
not mention the loss of speech as attending hysteria in men, particularly mentions the violence of the other symptoms. In his Medicina Nautica, Vol. II. p. 29, he remarks, "We found not a few of the cases sent to the hospital ships, subject to very frequent fits of hysteria, and where this singular affection occurred, with as much violence of convulsion as we have ever marked it in female habits, attended with globus, dysphagia, immoderate risibility, weeping, and delirium," &c.

Forres, December 6, 1814.

X.

Case of Aphonia occurring during the Eruptive Stage in Measles; and two cases of Contraction of the Fore-arm and Hand removed, by the use of a Splint and Bandage. By EDWARD SCUDAMORE, Member of the Royal College of Surgeons, London.

JAMES SIVEYOUR, of Sandhurst, in Kent, æt. 25. of a robust habit, was attacked by measles in autumn last, by exposure to the air on a cold damp day; the eruption was suddenly repelled, and he immediately lost his voice. In this state he remained for nearly five months, without being able to make himself understood, even by a whisper; he complained of a constant pain shooting through to his back, in the direction of the diaphragm. He applied to me about six weeks ago. I desired a blister to be laid on the epigastric region, and gave him volatile oleaginous medicines. The former did not appear to afford any relief; the latter he continued about a fortnight, and then coughed up a considerable quantity of viscid phlegm and coagulated blood; his power of speech instantly returned, and he at once became well.

MARY ——— æt. 17, the servant of the rector of this parish, was bled in the beginning of October last by a labouring man. The tendon of the biceps muscle was wounded, and contraction of the fore-arm followed. In a few days inflammation, extending to the shoulder, affected the whole of the elbow-joint. She applied to a neighbouring practitioner, who reduced the tension and inflammation by sedative applications, but the contraction of the joint remained for some weeks after. I was then directed to see her, and immediately used the unguentum hydr. c. camph. upon the biceps, and gradually brought the arm to its proper degree of extension, by the daily application of a roller and long splint, reaching from the fore part of the axilla to the wrist. In the course of a week a visible alteration took place, and she regained a free use of her arm at the end of a fortnight.

Succeeding so well with the use of the splint in the above case, I was led to adopt the same mode in removing an obstinate contraction of the wrist and fingers, in the case of a lady at Beckley, in this neighbourhood, which had followed a long illness. They had remained firmly clenched for many months. The fingers were at first raised by a firm compress of lint till the extremity of the splint could be placed under them; a small splint and roller were used for the thumb. The integuments on the back of the hand had been distended to a considerable size, but yielded readily to the use of the ointment. In about five weeks, the splint and roller were discontinued, and she has now the perfect use of her hand.

Northiam, Sussex, April 25, 1815.

XI.

Case of Tetanus, in consequence of a Wound, successfully treated.

By Dr Reid, Kilmarnock.

February 10, 1815.—James Macrae, aged 15, was thrown from and trampled on by a horse. Seven inches of the left tibia were laid bare, and the muscles on both sides opened up wide and deep. The bone was free from injury. After cleansing the wound, it was stitched up and the leg again brought into shape.

The sore was regularly dressed, and promised to heal kindly. On the 21st, however, the patient seemed remarkably peevish, and a peculiar irritability was expressed in his countenance, which wont to be placid and serene. The surface of the wound was still clean and healthy; but fearing the attack of a more dangerous disease, grs. iv. submur. hyd. were ordered to be given morning and evening, and the leg to be dressed with ungt. hyd. fort. that the system might as soon as possible be affected; and to this precaution, in great part, there is, I think, reason to suppose, was owing our success in the sequel. On removing the dressings on the morning of the 25th, instead of healthy pus, as before, the surface of the wound was covered with a darkish unhealthy-looking matter, which I had, in two former instances, noticed as the forerunner of tetanus. He complained of pain in the throat, and at the lower end of the sternum. The sore was dressed with ungt. Resinos. and ℥ss. ungt. hyd. fort. was ordered to be rubbed into the throat. The mercury had by this time begun to take effect, but to day it caused a troublesome purging; pulse 90 and hard.

26th.—This morning, whilst inspecting the fauces and depressing the tongue with a spoon, spasm seized the muscles of the jaw, and the spoon was held fast between the teeth. This went off in a little, and did not return at this time; but in the course of the day he had a few attacks of the kind. Even now the mouth could not be opened so wide as usual. This evening ℥ss. ungt. hyd. was again directed to be rubbed into the neck and breast, and the purging continuing last night and to-day the calomel powders were stopt. Pulse as yesterday; skin hot.

27th.—Last night he slept none. The back and neck are become very painful and stiff, and pain at the sternum severe; the spasms frequent and strong; he perspires considerably, and has much thirst. He describes the pains as shooting down his k, and thence up to his breast. They come on about every e minutes, when he screams out in agony. He had bit his tongue several times during last night. He lies now rigidly extended or rather bent back; swallowing is very difficult, and the jaws nearly closed; the purging is stopt. An opium pill of grs. ii. was directed to be given every second hour, when swallowing was possible, ℥ii. ungt. hyd. fort. to be rubbed into the thighs and back before night, and as much wine as possible to be given. The salivation is considerable; pulse 120, and hard.

28th.—The rigidity continues to increase, nor does it relax when the spasm ceases, except a little in the muscles of the jaw; and on any noise startling him, or a person touching or speaking to him, the spasms are instantly renewed. Two pil. op. only could be given during slight remissions, but the rubbing was persisted in all night. Salivation continues, and copious perspiration. Relaxation of the muscles to be watched, and grs. iv. of opium to be given, if possible, at once. The rubbing in to be resumed and continued at short intervals. From this date, the ungt. hyd. was in no stated quantity. The box was filled as soon as emptied, but not less than ℥ii. was used in the day.

March 1st.—The opium was taken, and ointment rubbed in last night as directed. The spasms came on at regular, but rather longer intervals to-day. After giving grs. iv. of opium, he slept half an hour, but was awaked by a strong spasm. In the intervals he is inclined to sleep; says the pains arise lower down than yesterday, and are not so violent; pain at sternum not so great. Having had no evacuation of any kind since the 27th, a glyster was ordered, which gave great relief, and he passed a large quantity of urine. After this he felt so easy, that the use of the opium was suspended till evening; two grains of which, since the exhibition of the first dose last night, had been given every second hour.

About eight o'clock, the spasms became exceedingly severe, and the use of the opium was again begun and carried on as before. The wine was given in large quantity, and the ungt. hyd. rubbed into the groin. The salivation, though considerable, was still not what might have been expected; ℥ii. ol. ricini was now ordered for the patient, in divided doses, and powdered opium was mixed with ungt. hyd. The pulse yesterday and today as on the 27th.

2d.—The ol. ricini had operated well last night, and its action continues to-day; the spasms seem to decrease in number and strength. During an interval of half an hour he had a little refreshing sleep. Has a little appetite. The medicines to be given as before, except the opium pill, of which he is to have one every third hour. Pulse 100.

3d.—To-day the patient does not seem so well as during the last two days. The opium failed in quieting him last night, and he had no sleep. The purging continues, but much diminished. He takes the wine still in large quantity, but is very weak. The back and neck are stiff, but the jaw pretty free, and swallowing easier; salivation considerable; the opium, ointment, and wine to be continued as before; pulse feeble and irregular.

4th.—Slept better last night, and feels easier to-day; had no attacks of spasm for three hours before I saw him; belly moderately open; stiffness of the neck and limbs is decreasing; salivation as yesterday. Continue the wine and medicines. Pulse 98, regular and firm.

5th.—Had but a few spasms during the afternoon of yesterday and last night, in one of which he again bit his tongue, but slept better than since the disease commenced. Suspend the ung. hyd.; also till evening the pil. op.; continue the wine. Towards night he complained of weakness, and the pulse became irregular. The jaws too became again rather stiff; ℥i. ol. ricini was given, and the use of the opium resumed.

6th.—Had a better night than was expected, and feels stronger to-day; rigidity diminishing; the ol. ricini has had no effect. Omit the pil. op. and repeat the ol. ricini, and continue the wine. Pulse pretty firm; perspiration greatly lessened.

7th.—Slept calmly several hours last night, and feels better, but very weak; physic had operated twice. Continue the wine.

8th.—Remains as yesterday.

9th.—Had a very restless night, and is much worse to-day. The spasms have returned with much force, and have been very frequent since five o'clock this morning; belly again costive. Ordered ol. ricini ℥iss. and pil. op. iii. to be given at once. Saw him in the evening; the spasms were become easier and less frequent since taking the above dose, which has operated twice. Repeat the pil. op., as usual, every third hour during night.

10th.—Slept little last night, but greatly better to-day; spasms more distant and easy; ℈i. Pulv. cort. cinchon. to be given in his wine four times a-day.

11th.—The usual quantity of opium had entirely failed in procuring sleep last night. He had several severe attacks of spasm, and this morning they are frequent and strong. Pil. op. iv. were given at once, which put an end to them at this time. A dose of ol. ricini was given soon after; and from this date they gradually declined in frequency and force till the 18th, after which they entirely disappeared.

The remarkable inveteracy of the disease in this case struck me greatly. The slightest suspension of the medicines was certainly followed by the recurrence or aggravation of the symptoms. The paroxysms, indeed, were generally of no long continuance, nor were the jaws for any length of time at once locked, except on the night of the 27th, when the spasms seemed to follow with such frequency and force, that for several hours the teeth were fast jammed. I observed, too, that when the disease was most violent, attempts to swallow seldom failed to induce the spasms, which were as certainly brought on by any trifling noise or disturbance near the patient.

The wine and opium given during the whole of the disease, neither affected the head nor induced sleep; although, in the space of nine days, twelve bottles of good port wine, and ten scruples of opium were taken, besides three drachms rubbed in with the ointment. Twenty ounces of the strongest mercurial ointment, in the same space of time, were rubbed in, conjoined, as I have said, with powdered opium. The salivation, notwithstanding, was never great. The mouth indeed was affected early in the disease, and this was kept up throughout; but the addition made to it by the rubbing in, seemed inconsiderable, when compared with the quantity of mercury used. But the resistance opposed to medicine by this disease is amazing, and only to be believed by those who have seen it tried.

I began the use of mercury early in this case, from a conviction of its utility. I had before this seen life apparently saved by its timely and steady use; and few will put the effects of mercury in the balance with that of tetanus, which in far more than the majority of instances is death.

A case of the same kind, ending fatally on the sixth day, occurred to me only four months before; and from this and two former instances, I had learnt how very inefficient any medicine is apt to prove, after the disease has attained its strength. By these, too, I was warned to watch narrowly the most distant appearance of so formidable and unmanageable a disease.

XII.

Case of Convulsions after Delivery. By THOMAS DUNCAN, Surgeon, Grenada.

MRS ———, aged about 22, stout, but somewhat deformed in her lower extremities, with a peculiar cast in the eyes, which gave them a heavy appearance, and rather a dilated pupil, her head also uncommonly flat on the top, fell in labour of her first child on the 23d April 1811, which was tedious and lingering; she was, however, safely delivered on the morning of the 24th, the natural discharge following the placenta, &c. A pain in the head, which she had been subject to before her marriage, and during her pregnancy, troubled her in the early part of the day, with dimness of sight; and about two o'clock, P. M. she was suddenly seized with general convulsions, which threw her out of bed; another fit succeeded in half an hour, and rendered her totally insensible. They continued to recur at irregular intervals, increasing in duration and severity until 6 o'clock, when she was first seen. The coma was then complete; breathing hurried and stertorous; pulse 140, regular and firm; skin moist, and rather warm. A fit came on, during which the muscles of voluntary motion were violently agitated, the countenance distorted, and she foamed at the mouth; others followed of longer continuance and greater severity, and the breathing became more laboured. A vein was opened and about ℥xxx. of blood taken from a large orifice; the head was shaved, a blister applied over it, and a purging glyster given. Relief immediately followed the venesection; the breathing, although it continued stertorous, became less hurried, and she was not so restless. At 9 o'clock the number of fits was eighteen, but the two last were milder.

During the night, the respiration became nearly natural; the pulse, however, increased to 160, and was very weak. Lochia stopped. Another blister was applied between her shoulders, and the camphorated emulsion ordered every hour, with warm applications to the abdomen.

25th, 4 o'clock A. M.—No fits, but the stupor continues, and restlessness is increased; breathing easy; puts her hand sometimes to her head, at other times to the lower part of her belly. Opens her eyes, the pupils of which are much dilated, but is insensible and cannot speak; pulse 140, weak and intermitting; had two small operations from the glyster.

Capt. statim. sulph. potass. ʒij.
Cont. emuls. camph. et lanul. calid. region. uter.

12 o'clock.—Having vomited the sulph. potass. and with it a worm, another dose was administered, which procured four or five motions. Some lochia. The blisters have risen but partially.

8 o'clock, P. M.—Easier and somewhat sensible; no return of fits; pulse 120, and fuller; skin moist; lochia more abundant. Cont. med.

26th, A. M.—Perfectly sensible and much better; slept well; is quite ignorant of having been delivered, and of every thing that had happened; pulse 108. 8 P. M.—About noon complained much of her head, and felt drowsy; pulse 120, and skin hot; ʒij. sulph. potass. were dissolved in chicken soup, but she could not be prevailed on to take it; had therefore gtt. xx. vin. tart. antim. every hour, and a purging glyster, which operated. She is now easier, but the headach is not gone.

Omitt. emuls. camph.—Cont. vin. antim.

27th, 6 A. M.—Slept pretty well, and, though sensible, is confused, and wanders occasionally; headach increased, and eyes painful; pulse 120, and hard, skin warm; T. foul.

Emitt. sang. ven. jugul. ad. ℥xii.
Capt. infus. senn. ℥i. omn. hor. donec. respond. alvus.

7 P. M.—Fainted from the bleeding; headach relieved; six operations from the purgative; is cheerful, and feels better than she has yet done; pulse 120; skin cool; some pale lochia.

28th, 6 A. M.—Slept almost the whole night; lochia scanty; pulse 114; declines taking any medicine, as she feels so well.

6 P. M.—About 11 o'clock was seized with a convulsive fit, which lasted full three minutes; during it she bit her tongue. In half an hour another came on, and they continued to return at irregular periods; up to this hour 13 have been counted. The muscles of the left side were considerably more affected than those of the right; the left thumb, for the first time, was firmly clenched in the middle of the hand, and the fingers closed; the right hand continued open and free. The eyes were also drawn to the left, and the pupils much dilated, and insensible to the stimulus of light. After two or three fits she became unconscious of every thing, and now remains so. Before the attack, the headach had returned with great severity, and she expressed a desire to be blooded, having before found relief from that operation, ℥x. were therefore drawn and a blister reapplied to the head.

℞ Tinct. valer.
Æth. vit. āā ℥ss.
Syr. simp. ℥i. Aq. ℥vj.
M.—C. cochlear. ii omn. hor. durant. convulsion.

29th, 6 A. M—Had three slight fits before midnight; sensibility returned, but there is a considerable degree of coma, and doses much; pulse 120; some lochia; occasional convulsive twitches of the eye, which draw it to the left

Capt. pil. ammon. cupr. i. ter indies.

6 P. M.—Vomited a considerable quantity of bile, after taking the pills; had a glyster which brought away some scybala; pulse

ranges from 114 to 120, and strong; is pretty sensible, but the twitches of the eyes continue. B. costive.

℞ Calom. G. gambog. āā gr. iii.
Pulv. jalap. ℈ss.
Ft. bolus cap. stat. et. rep. om. hor. donec alv. sol.

30th, 8 A. M.—Had three boluses, and as many glysters, from which she has had five copious evacuations; no fits, but still confused, and a little headach; pulse 108.

Rep. pil. ammon. cupr.

7 P. M.—Complains of her head; has taken three pills to-day; belly open; pulse 108; lochia trifling.

1st May, 6 A. M.—Slept well; no fits; some headach; pulse 102.

Aug. dos. pil. cupr. ad. ii. ter indies.

Six P. M.—Better; pulse 100; no stool to-day.

Capt. infus. senn. u. a.

2d, Eleven o'clock, A. M.—Slept well, but had a severe headach this morning, which was removed by bleeding to the extent of ℥x.; two operations from the medicine, but complains of heartburn.

Contr. pil. cupr. et capt. Mag. calcin. ʒi.

3d.—Slept well; no headach, but her understanding and memory are far from being perfect; pulse 105; B. open.

Contr. pil.

8th.—Since last report she has been mending. A slight headach comes on in the morning and increases towards noon, for which she has been using ablution of cold water with advantage; recollection much impaired; the pupils of the eye are more dilated than before her confinement, and, besides a dimness of vision, there is a vacant stare; takes the pil. cupr. regularly.

From this period she gradually got better. The headach abated by degrees, and her mental faculties improved, but she ever remained unconscious of having given birth to a child, which in a little time she was enabled to nurse without much inconvenience. Since that time she has had another child, with no further indisposition than occasional headachs, which were relieved by bleeding and laxatives.

In reviewing the preceding case, I think there may be traced a strong tendency to sensorial derangement from the severe headachs, &c. and it is a matter of doubt whether some predisposing cause, as denoted by the flat head and dilated pupil, may not have existed. The exciting cause seems to have been violent irritation, the consequence of a long and painful labour. The disease in the beginning more nearly resembled eclampsia than simple convulsions; and in the end it evidently terminated in epilepsy. The treatment, as will be seen, consisted chiefly in bleeding and purging, from which the advantages were so great, that the patient herself applied for the former when she dreaded a relapse, or felt a severe headach. She was blooded four times in the course of eight days, to the extent of ℔v., to which, more than all the other remedies put together, I attribute her recovery, but especially to the first; and I owe it to my brother Alexander, to acknowledge that this step was the result of his sole judgment, before I had an opportunity of seeing the patient. I forgot to mention that some of the relations gave her about 30 gtts. of laudanum on the 28th, but they had no effect in allaying the convulsions.

XIII.

A Case of Labour, with Apoplexy, Convulsions, and Flooding. By G. F. Edwards, Member of the Royal College of Surgeons.

On Monday, December 19, 1814, a young married woman, the wife of Jacob Poulsum of this city, about 20 years of age, was taken in labour with her first child. The pains continued at regular intervals for several hours, when the membranes ruptured. She sent for the female accoucheur who was to attend her. The woman arrived, and, after waiting some time, she made an examination, and assured her patient that every thing was going on regularly and naturally, and led her to believe that she would be soon released.

This deluded and self-conceited midwife, ignorant of the real state of the case before her, allowed the poor young creature to go on suffering from day to day the most exquisite torture, without any prospect of relief. She was continually pressed by the friends of the woman to have the assistance of another practitioner; still she persisted that things were natural, that time must be allowed, and that she could do. At length, however, she was compelled to relinquish the case altogether in consequence of the approaching symptoms, indicative of the most imminent danger, and to urge the immediate attendance of a surgeon.

The labour pains continued uninterrupted from Monday at noon until the following Thursday, when Mrs P. complained of a violent pain in her head, and dimness of sight, which was soon succeeded by apoplexy, convulsions, and paralysis of one side. She continued in the above alarming state from Thursday night twelve o'clock, until the following morning, Friday at eleven, when my attendance was requested.

When I saw this poor young creature, she was extended on a bed in an apoplectic state, just recovering from a violent convulsion, which appeared as though it would terminate her existence. One side was paralysed, her breathing was stertorous, and the pupils completely dilated. In this alarming situation I found she was not delivered.

I made an examination per vaginam, and found the os uteri nearly dilated, and one of the superior extremities presenting; the arm being bent upwards, the elbow was thrust into the vagina, and the continued action of the uterus had forced the shoulder so tight between the sacrum and pubes, that made it impossible to insinuate my finger at any one point.

Considering that little was to be expected towards her recovery unless delivery could be effected, I made an attempt to force the projecting arm upwards, in order to gain room to pass my hand into the uterus, when I was surprised by a gush of blood. At this moment a violent convulsion coming on, it obliged me to abandon the project I had in view of turning. I was not intimidated by the hæmorrhage, and though I was conscious that I had not then the power of controlling it, yet I felt convinced that, under the existing circumstances, it would prove beneficial. I repeated the effort in order if possible to gain admission into the uterus, and the same effects followed. At length, however, I succeeded, although it was attended by an immense gush of blood proceeding from the placenta, which was attached at the cervix uteri, and partially separated in the attempt to force the shoulder into the uterus. I was not long in finding the feet; and when the child was turned, and the body brought into the vagina, the hæmorrhage ceased; the arms and shoulders soon followed the extremities and body through the soft-parts. The head now presented a difficulty; as I found the usual means ineffectual, and as I was fearful the head might be separated from the neck, the forceps was introduced, and the delivery completed. When the funis was divided, the hand was again introduced into the uterus, and the placenta separated from its adhesion, and brought away entire, without any particular hæmorrhage.

My attention was next directed to the mother;—having saved the life of the child, I was desirous of saving that of the parent. Her pulse was at this time 130 in a minute, full and vibrating;

pupils dilated; the left side, viz. the leg and arm, motionless, particularly at the time when convulsions came on. I opened a vein in the arm, and took away twenty ounces of blood from a large orifice. The pulse diminished in frequency under the bleeding, but the volume of blood striking against the finger continued as great. Soon after this loss of blood, a violent convulsion came on, which threatened immediate dissolution. This, like those which preceded it, ceased; and, finding the action of the heart and arteries excessive, I opened the temporal artery, and extracted fifteen ounces of blood in a very short time. This second bleeding diminished the frequency of the convulsions. Perceiving this favourable change, I raised her head upon pillows, and left her with strict injunctions not to be disturbed. In two hours my visit was repeated. In the interval I found my patient had had two convulsions, neither of which was so violent as before the bleeding from the temple. Her pulse was at this time 120 in a minute, and full; pupils dilated. I observed that neither the venous nor arterial blood was buffy; the breathing was more natural; the vein in her arm was reopened, and about ten ounces of blood taken; an injection ordered to be administered; and some aperient medicine given, if sensibility returned. I visited her again about eight in the evening, and found that she had not had a recurrence of convulsions. Her breathing was quick, but not laboured; bowels not open. I forced her mouth open by the aid of a spoon, and succeeded in getting into the stomach a three grain calomel pill, and a black draught. The last blood was buffy; the pulse 120 in a minute, but small. I opened a vein in the arm, which had been paralysed, and, when the blood flowed, perceiving it to be almost colourless, I desisted, and tied it up. I ordered a blister to the nape of the neck, and, giving instructions to continue the aperient medicine until the bowels were freely evacuated, I left her for the night.

Saturday the 24th, I found my patient much better; no return of convulsions; breathing more natural; pulse 100 in a minute; pupils more sensible to the admission of the light; bowels open; sensible when roused, but relapsing again into a state of stupor. In the evening I found her still better; bowels open; pulse the same as in the morning. The blister had performed its office satisfactorily; and the intervals of sensibility were longer and more defined.

Sunday, 25th.—Night passed in tranquillity; bowels open; pulse 98; sensibility greater. She had taken a few spoonfuls of gruel, and had asked to see her child. In the evening she complained of her head.

Monday, 26th.—Night restless; complained of a violent pain in the whole extent of the forehead; pulse 100 in a minute; tongue white, and much furred; bowels not open. Ordered her a cathartic mixture, and a pill of calomel. In the evening she was better; her breasts were filled with milk, but not sufficient to give uneasiness, or to make it necessary to have them drawn. She still complained of her head, I was desirous of applying leeches to the temples, but they were deferred until the morning, hoping, when the bowels were opened, the above symptoms would subside.

Tuesday, 27th.—The night more tranquil; bowels freely evacuated; pulse 97; tongue cleaner; thirst not so urgent; head much better. Desired the breasts to be drawn, and only to take gruel and barley-water. The uterine discharge was natural; and the urine passed easy, and in sufficient quantity.

Wednesday, 28th.—Night passed in sleep; pulse 90; bowels open; the pain in the head nearly subsided; milk very freely secreted; the child had sucked; and I was happy in pronouncing my patient this day out of danger, and from this time she continued gradually to get strength.

Remarks.

It is probable that, on Monday night, the 19th, she might have been delivered by turning, (as the membranes ruptured at that time) even admitting the placenta to have been attached near or over the os uteri, and a considerable hæmorrhage to have ensued. In that case the same reasoning would have obtained, and the same practice have been more *urgently* called for. It is true more difficulty might have been encountered at that time, in consequence of the continued action of the uterus interfering with the process of artificial dilatation; and more danger would most probably have ensued from excess of flooding, but neither of these reasons should, for a moment, have overruled the necessity of a speedy delivery by turning, or be adduced as a cause for any surgeon shrinking from his duty.

If she had been delivered on Monday night, we are warranted in concluding that apoplexy would not have supervened, as she had not complained of any pain in the head prior to the commencement of labour, nor were any symptoms present at that period indicative of such an affection; and, further, the pain in the head was not complained of until Thursday, the 22d, three days from the first attack of labour; therefore we may conclude that the pain in the head, apoplexy, and convulsions, were induced by the unwearied and ineffectual action of the uterus, and by the untoward position of the child, pressing upon the large descending blood-vessels, and causing increased determination to the brain.

In this case the alarm caused by the flooding was, in great part, done away by the presence of apoplexy, because loss of blood, from any part of the system, was desirable under such circumstances; and the only thing to be lamented in the first effort at turning was, the violence of a convulsion, which obliged me to withdraw my hand. The second attempt, however, was successful; and yet we find, when the pressure was removed, that no amendment in the alarming symptoms was observed, as the convulsions recurred as frequently; nor did any abatement take place until thirty-five ounces of the blood had been taken away, in addition to that lost from the uterine vessels.

It may be presumed, that some effusion had taken place in the ventricles, as there was evident paralysis of one side for a time. Whether this effect was produced by a rupture of a small vessel, from serous effusion, or transudation, I am not competent to determine. The other symptoms were correspondent with such a supposition, such as stertorous respiration, dilated pupils, strabismus, and foaming at the mouth, particularly at the time of a convulsion. And what most satisfactorily proves this to have been the case was, the removal of the pressure by the loss of so much blood.

The favourable termination of this case clearly demonstrates how much may be effected by patient, yet resolute, perseverance; and also shews that, under the very worst apparent circumstances, things sometimes turn out favourably when least expected.

Bath, February 2, 1815.

XIV.

Case of Reunion of the first Phalanx of the middle Finger, communicated in a Letter to Dr William Balfour, Edinburgh.

By Henry W. Bailey, Esq. Surgeon, Thetford, Norfolk.

Sir,—Although I am not personally known to you, yet I have ventured to address you upon a subject which, from its nature, may not be unacceptable to you. While perusing your paper in the Edinburgh Journal for October, upon the reunion of parts completely separated, I was called to an accident of a similar kind, occurring to a labouring man at Auxton, a village about two miles from Thetford. He had been supplying a chaff-machine worked by a horse, and entangled his middle finger with the knives, by which the first phalanx was completely cut through at the middle of the bone, and had been separated nearly an hour and a half before I saw it. The joint was not in the least injured. I was willing to try how far reunion of the parts might be effected; and, having cleansed the parts, I reapplied them carefully together, and secured them by plasters and small pieces of cards by way of splints. I desired the man to call upon me in a week's time, fully expecting to find the end mortified. To my astonishment, reunion had taken place, pulsation was felt distinctly at the end of the finger, and its colour was healthy; the nail separated at the fortnight's end; he complained of numbness in the finger. I have pleasure in saying the reunion was completely effected in about five weeks from the accident. The wound healed by the first intention, and only required three or four dressings. I saw the man yesterday: he felt no pain in the finger. It is as strong as the others, and the bone is perfectly united, but he has not the power of bending it with the other fingers. The nail, I understand, had been bruised some time before the accident, and probably would have separated had not he met with the subsequent occurrence.

I do not communicate the above case merely from ostentation, but from a wish of adding another fact to those you have already given to the public.

Any use you may think proper to make of this case, if deserving of notice, it is perfectly at your service.—I am, &c.

Norfolk, Thetford, May 12, 1815.

XV.

Observations on the Necessity and Utility of Blood-letting in Continued Fever, more especially in the Ardent Fever of the West Indies, commonly called the Yellow-Fever. By JOHN ALLAN, Surgeon, R. N.

THE benefit resulting to the science of medicine from the theories invented by eminent men, have never been altogether free from some inconvenience. Those theories which account for the debility attendant on the attack of continued fever, by deriving it from a direct failure of nervous energy, appear to have been productive of a great evil in practice, by impressing on the mind an unnecessary fear with regard to the requisite evacuations, especially that of blood-letting; for, though this was successfully employed by the more ancient practitioners, it seems at present to be too much neglected. Perhaps the confidence that has of late been placed in the free admission of cool air to the apartments of the sick, in the application of cold water to the surface of the body, and in the liberal use of purgative medicines, has so much engrossed the attention of medical men, as to make any other practice generally disregarded, and to cause to be overlooked the bold and laudable attempts of some individuals to vindicate the superior utility of free and extensive blood-letting.

It is admitted, even by those who derive the debility in fever from a failure of the nervous energy, that blood-letting may occasionally be beneficial, and rules are usually laid down to regulate its use; yet it is the most general doctrine, that the practice is seldom admissible, and that the repetition of it is hardly ever justifiable in fever, particularly in those fevers which appear in Britain. At the close of my medical studies at Edinburgh, about seven years ago, I was quite confident in this general persuasion, and fully contented in the belief that an emetic, free purging, the cold affusion, and what is called the cooling regimen, constituted the most essential means required at the commencement of continued fever. I thought, that a vigorous application of these means on the first attack, was almost sure to prevent the dreadful symptoms of debility and putrescency in the latter stages, the frequency of which I ascribed to the neglect of the proper measures at first, to the malignant nature of the epidemic, or to peculiar circumstances in the constitution or situation of the patient.

After embracing these opinions with the partiality naturally due by inexperience to the doctrines of respected teachers, it was not without surprise and distrust that I soon afterwards heard it asserted, that large and frequently repeated blood-letting was not only safe, but absolutely necessary, to the successful treatment of the most dangerous fevers,—that there was no such thing as typhus,—that every fever, at its commencement, absolutely admitted of being cut short within thirty hours,—and that a practitioner had only himself to blame, if, when called at first, he allowed any fever to assume a typhoid form. I have no doubt that the rehearsal of these assertions will be equally startling to all who have not paid particular attention to this subject, as they were at first surprising to me; but, although I have mentioned these opinions almost in the very words in which I first heard them delivered, I am not prepared to defend them to the full extent. The cases of continued fever which have since come under my observation, have not much exceeded forty; but as, in the treatment of these, I have seen facts sufficiently striking to justify conclusions favourable to the most active antiphlogistic measures, utterly irreconcilable to any other system of treatment, and producing a conviction, on my mind, altogether unexpected, I think a detail of these facts cannot fail to be useful to the public. I therefore submit the cases to the world, and leave them to justify or to condemn the conclusions I have drawn.

As the object of communicating these cases is simply to exhibit the admirable effect of blood-letting in continued fever, so as to draw the attention of others to the subject, and to prove the indispensable necessity of adopting the practice, especially in the yellow-fever, I do not, in these observations, attempt to inquire particularly into the advantages to be likewise obtained from other means, being quite convinced, that, without blood-letting, no other remedy, however beneficial, when used as an auxiliary, can with safety be depended on. Before detailing any of the cases in which I have observed the great advantage of blood-letting, I may state, that when I found the opinions favourable to this practice openly professed, but not publicly condemned;—boldly and sincerely asserted, but not attempted to be denied or refuted,—while, on the other side, it was admitted that blood-letting might occasionally be advantageous, I resolved to resort to experience as my guide, to be led to a decision only by the most positive facts, and therefore to pay particular attention to blood-letting in any cases of fever I might meet with. My services, till last year, being confined to ships in the English Channel, I have had but few opportunities of making observations of this kind in Europe. Indeed I cannot say that I have since seen any case of continued fever, and therefore I do not pretend to assert from experience that the practice recommended is to be extended, without some caution, to fevers arising from contagion. Judging however, from analogy, from what I recollect having seen of such cases in the more early part of my professional life, and from the authority of the celebrated Sydenham, I cannot doubt that blood-letting, the effects of which are so very different from what those educated in the doctrines now in fashion would expect, will at last be found to be consistent with nature, and to be the best and most successful method of treating all fevers, even the plague itself. Tho two following cases, which occurred in Europe, the one at Portsmouth, the other at Helvoetsluis, served with some others to confirm in my mind the impression in favour of these opinions, and to impair the confidence I at first had placed in the mode of treating continued fever, usually pursued in Britain.

CASE I.

" JOHN HOGG, (seaman), 21 years of age, of a slender habit, was attacked, on the 20th May 1811, with shivering, sickness, vomiting, headach, and general uneasiness. His bowels were that day purged with senna; he was bled to the extent of 14 ounces, and in the evening he took an emetic. During the night he perspired freely, and felt himself, the following morning, almost free from complaint. In the course of the day, however, the headach returned; and, in the evening, had increased to a violent degree, being attended with a sense of cold, violent and sudden starting, and derangement of the mental faculties. I found him tossing about in bed, with a great tendency to vomiting, complaining very much of his head, neck, and eyes. His skin was extremely hot, though he complained of cold; his pulse was slow and firm; his lips and tongue parched and furred; and he took drink very greedily when offered to him, though he said he felt no thirst.

" Twenty ounces of blood was taken from his arm, and then he took an emetic, which brought up a quantity of crude ingesta. About an hour afterwards, the delirium appeared to be returning, and 25 ounces of blood were taken from another vein. It was of a florid colour, and came jet by jet from the vein, though situated at the radial side of the arm, and on that account was not liable to be affected by the pulsation of the artery. Towards the end of the operation he opened his eyes, and expressed himself relieved. A large blister was then applied betwixt his shoulders, and he soon fell into a very calm sleep, and was covered with a gentle perspiration.

" Early next morning, although he declared himself much better, and was free from any appearance of delirium, I thought it necessary, on account of the state of his pulse and of his eyes, to take 30 ounces of blood from his arm, which had a very tough firm coat, of a pale blue colour.

" At noon he was conveyed to Haslar Hospital, complaining of nothing except debility, which, however, was not so great as to prevent his walking without any assistance from the boat to his ward.

" He returned to the ship after a few weeks perfectly cured."

CASE II.

"April 14th 1814.—SAMUEL ARMSON, seaman, aged 19, complained of violent headach, pain in his loins, great thirst, universal uneasiness, and was affected with a degree of delirious incoherency, expressing a sense of fear and suffering, and scarce being able to return suitable answers to questions. He had been affected the day before with nausea, a sense of cold, and pain in his loins and head. He had taken a scruple of ipecacuanha, which had brought up a quantity of bile; and 16 ounces of blood had been taken from his arm.

"In this state, a like quantity of blood was taken from his arm. On the 14th he took four grains of calomel, and was ordered to have small doses of Epsom salts every hour. By six o'clock in the evening, his bowels had been well purged, but he still complained of great pain, and was not perfectly rational. The blood-letting was repeated as before; and he took a dose of antimonial powder.

"On the 15th April, his mind was quite collected; he had less pain in his head and loins, and his bowels were loose; but he complained of a fixed acute pain in the left side of his breast. A vein was again opened; but, owing to the approach of syncope, only a few ounces of blood could be procured. His bowels were well purged through the day, with small doses of the sulphate of magnesia; and in the evening he had only a slight pain in his loins, and a general feeling of uneasiness. He took a dose of antimonial powder, with one grain of calomel, at bedtime.

"For several days afterwards, he complained of pain in his loins, and required repeated doses of the salts or of calomel, to keep his bowels open; but finally returned to his work, perfectly cured, on the 25th of the month."

I am aware that the first of these cases may be considered, by those who will not admit the propriety of blood-letting to such an extent in fever, as a case of phrenitis; but there can be no dispute, whatever name we bestow on it, that the disease was actually cut short by the vigorous practice pursued; and, if we are to call it either phrenitis or pure synocha, we cannot withhold the same appellation from almost any case of the yellow-fever. If the patient had not been bled, and had survived the disease, it would have exactly followed the course of one of the worst continued fevers of Britain, and the patient would in all probability have been several months confined by it. By his own account, he had some years before had an attack of what he called brain-fever, which confined him many months. I know that he had such a disease in 1812, when in a prize-vessel at a port in Ireland, where he was confined to bed nearly two months. In 1813, he was threatened with another attack, which was cut short by one large bleeding, and the other usual means.

From the difference in the accounts to be found in books on the yellow-fever, I was, as it were, in suspense and uncertainty how to act when this ship was sent last year to the West Indies; but, having, in the space of two months, met with five and thirty cases of that disease, in a crew consisting of 130 men, I have seen ample reason to decide in favour of the most vigorous antiphlogistic measures.

This disease frequently commences with symptoms exactly similar to those described in Hogg's case; yet, when it proves fatal, besides the peculiar yellowness of the skin, and the vomiting of a black coffee like fluid, it is known to exhibit all the symptoms of debility and putrescency with which our descriptions of typhus (gravior) abound. Again, when it terminates successfully, the patient frequently, as was the case with Hogg, is not for a single day entirely confined to bed, and is commonly restored to health in a few weeks. Very few survive, when what is usually called the second stage has come on, that is, when essential relief has not been obtained within three or four days. But when the proper measures have been adopted at first, there very seldom appear symptoms, either of putrescency or of remarkable debility after that period. If I were to speak entirely from my own limited experience, I should have said that the symptoms of extreme debility ought never to appear in the yellow-fever; for I have met with no instance of it, except in two cases, which proved fatal; and, with respect to both of these, I must candidly express my persuasion, from what I now know, that both would, in all probability, have been saved by as liberal and extensive a use of the lancet as was practised in the other cases. One of them was partly, the other wholly, under my own management; and in both I had occasion to observe, with extreme but unavailing regret, the inevitable consequence of permitting, through inadvertence or mistake, the proper period for bleeding to pass, without taking the full advantage of it, in a disease which runs its course with such amazing rapidity as the yellow-fever. If, in such circumstances, the effect of blood-letting is tried, either on account of the irritation of the stomach, affected with perpetual vomiting, or from a belief that it may relieve the head, it produces a transitory quiet; but, as a friend of mine describes it, " local congestion has taken place irremoveably, the patient looks like a man intoxicated, staggers and faints, his pulse wants tone, and, though venesection gives temporary relief, the symptoms of an oppressed sensorium always recur, and at last terminate in death."

Blood-letting, therefore, will rarely admit of being employed after 36 or 48 hours, from the attack of the fever; but, when used, it ought to be large, and repeated frequently, at short intervals, till it has relieved the symptoms.

If we examine the arguments against this practice, we find that we are cautioned not to be too free in the use of the lancet at the commencement of typhus fever, even in books pretending to give rules for the treatment of the yellow-fever, called Typhus icterodes, not on account of the debility existing at the time we prescribe it, but for fear of that which is to supervene in the latter stages of the disease. Nothing can be more absurd, nor productive of greater mischief in practice, than this doctrine is, as applied to the yellow-fever. Indeed it ought to be a maxim, that cannot be too strongly impressed on the minds of those who have to conduct the cure of this disease, that the debility resulting from the most copious blood-letting, employed in the early stages of the disease, is not to be compared to that which is the consequence of the disease, when allowed to riot unchecked for two or three days. The debility attending the attack of the disease, may truly be called indirect. It is like that debility in kind, though not equal in degree, which disables a man from performing all the actions of health, when he is comatose from pressure on the brain in apoplexy, or in accidental effusions of blood. The effect of blood-letting is to relieve this fulness of the vessels of the brain, to remove the pressure from the sensorium, and, in an indirect manner, to restore strength.

The debility which occurs in the latter stage of the yellow-fever, which is attended with yellowness of the skin, with inability to bear the erect posture without fainting, with tremors of the limbs, with subsultus tendinum, with involuntary evacuations, and with what have been considered as signs of putrescency, is absurdly imputed to the blood-letting, which cannot have been large or frequent, else these symptoms had probably never appeared. If, when the disease commences in its most violent and alarming form, the free and extensive use of the lancet restore the patient to convalescence, almost to health, within 24 hours, can it be said, that the blood lost in another case has caused the dreadful symptoms of debility enumerated above, where the disease at first was less violent, and where, perhaps on that very account, less vigour has been employed in the treatment, and less success found in the result? It would not be more unreasonable to argue against the use of the lancet in pneumonia, on account of the fatal debility attending cases of that disease, where effusion has taken place, or to ascribe that debility to the previous blood-letting. The debility so much dreaded, occurring at the end of the yellow-fever, is the consequence of violent inflammation having destroyed the most essential organs of life, especially the brain and the stomach. Effusions, and other marks of morbid action, have been found in the brain on dissection; and the stomach is admitted, on all hands, generally to exhibit evidence of the highest inflammation, and frequently to have been found in a gangrenous state. The inflammation has been found extending from the stomach to the intestines, to the liver, and even to the peritonæum and pleura of the diaphragm. Blood has been found effused into the cavities of the thorax, and into the cellular texture of the lungs; and all these morbid appearances have been observed in cases where blood-letting was not used. To ascribe, then, the debility attending such fatal changes of structure to the effect of blood-letting when it has been unsuccessfully applied, is erroneous and absurd.

The debility which occurs in cases of the yellow-fever, where blood-letting has been employed with the usual success, has never, in any case that I have seen, been near so great as what I remember to have often observed in others, and to have twice experienced in my own person, after attacks of the fever, commonly called in Britain Typhus mitior, proceeding from contagion. It resembles very nearly that debility which follows an attack of pneumonia, when it has been necessary to take away very large quantities of blood. It is generally soon removed by nourishing diet, although, indeed, there is always some risk of a renewal of the fever, if the appetite for food be fully indulged. There is a constant tendency to costiveness, and to a return of one of the most distressing symptoms of the disease, an acute fixed pain in the forehead, which requires a continued use of purgative medicines, and great caution in permitting the use of wine, and even of full diet, for some weeks after the fever has been subdued. As purgatives are invariably found absolutely necessary to prevent and remove these returns of headach, the best method is, to give two or three grains of calomel at bedtime, to be followed in the morning by half a drachm of the compound powder of jalap of the Edinburgh pharmacopœia, or half an ounce of Epsom salts, or fifteen grains of rhubarb. Sometimes it is even necessary to recur to the use of the lancet, (and it is resorted to with advantage) after the lapse of two or three weeks, in cases where its effects had been most salutary at the first,—a circumstance strongly proving the inflammatory nature of the disease.

I have already stated, that I have not seen any case of contagious fever; for, whatever doubts others may entertain on this subject, I am perfectly convinced that the yellow-fever is not contagious, from reasons which it may not be amiss here to explain.

This ship arrived at Jamaica in the latter end of October last, during an uncommonly sickly season. We remained in that harbour about three weeks, and in that time sent eight cases of fever to the hospital, all of whom returned on board before we left that island, most of them in a state of convalescence. After going to sea, the disease became more prevalent than it had been before, but its frequency and violence gradually diminished as we were farther and farther removed from the influence of the sun in the torrid zone, and had almost wholly ceased by the middle of January, when we reached the temperate island of Bermuda. There cannot, in my opinion, be a stronger proof that a disease is not contagious, than our having had so few affected, and the disease having disappeared so suddenly, affords, in this instance, with respect to the yellow-fever. Every person in the ship was unavoidably exposed to circumstances in which contagion usually acts; but the disease evidently arose from the scorching and intolerable heat of the air, or from exposure to the direct rays of the sun; and, in other ships which were longer at Port-Royal than we were, this was fully evinced, by every individual belonging to them being sooner or later affected with the disease.

Before concluding these remarks, and proceeding to the detail of the cases, I have only to express the hope that this subject will soon attract the attention which is due to it from those who are capable of supporting these observations by more numerous facts, and more extensive experience; for I know that many medical men are now in the habit of bleeding very extensively in the ardent fever of the West Indies, although there is hardly one of them who has not, as well as myself, been obliged to set aside the tuition of books and the prejudices of education, and who would not, before he went to the West Indies, have considered such a practice altogether unjustifiable. For my own part, I firmly believe that the practice of extensive blood-letting will soon be found equally beneficial in contagious, as it certainly is in sporadic fever; for if a disease, called in nosological books *Typhus icterodes*, be found, by experience, to consist in the most violent inflammatory actions, and to require a treatment so very different from what its name implies, it is but fair to presume that experience may prove the same to be the fact with regard to what has been called *Typhus gravior*.

The following is the first case of the West India fever which I met with. It occurred a few days after our arrival at Jamaica.

CASE III.

"October 26, 1814.—Mr BROWN, midshipman, aged 19, was suddenly attacked in the forenoon, while walking the deck, with giddiness, headach, sickness at stomach, and alternate chills and flushings. His thirst was immoderate; he looked as if he were frightened; and he could not rest a minute in any place.

"Twenty ounces of blood were taken from his arm. He spontaneously vomited a quantity of bilious matter, and soon after took 12 grains of calomel. This was followed by half an ounce of sulphate of magnesia every two hours, which at last, operating fully on the bowels, relieved every symptom like a charm.

"Next morning he took half an ounce of the salts, and the same quantity at night; and on the third morning, the salts having procured several copious stools in the night, he was almost without any complaint. He took a mercurial pill at bedtime.

"On the 29th, he complained of occasional returns of the giddiness and of nausea. He took a scruple of rhubarb in powder.

"The 30th, he still felt his head light and giddy, the rhubarb having had no effect till that morning. He took again half an ounce of the salts, and on the 31st he returned to his duty, free from all complaint."

Here follows the second case of this disease, the attack of which was about 12 hours after the preceding one, but attended with infinitely more alarming symptoms.

CASE IV.

"JAMES NAYLE, seaman, aged about 32, was found about midnight, 26th October, in a state of raging delirium, when he complained greatly of his head and of his bowels. His skin was excessively hot and dry; his tongue parched and white; and he drunk cold water with great avidity when it was offered to him, and anxiously called for more. His limbs were drawn up to his belly, which felt hard and unequal. His pulse was full, and about 80.

"In this state he took 15 drops of tincture of opium in an ounce of peppermint water; and 16 ounces of blood were taken from his arm. While it was flowing, he became faint and sick, and threw up the water he had drank, tinged green.

"He afterwards lay tranquil for some time, but his thirst continued very urgent, and his language incoherent. After the expiration of half an hour, 18 ounces more blood were taken from a vein, and, as he felt exceedingly sick at stomach without vomiting, he took a dose of ipecacuanha. After its operation, which took place fully in a few minutes, he took 10 grains of calomel, fell into a very profound calm sleep, and was covered with perspiration.

"At six o'clock next morning he complained of a pain in his left side, aggravated by a full inspiration. His head was free from uneasiness; his mind calm and collected; and his thirst far less urgent. His skin, however, still felt hot, and his tongue continued white, though not furred.

Eighteen ounces of blood were then taken from his arm; he took a dose of calomel and jalap; and was directed to take half an ounce of Epsom salts every two hours, until his bowels should be affected.

"By six in the evening his bowels had been fully evacuated, and he then felt the pain in his side only on making deep inspirations. He took four grains of calomel at bed-time.

"On the 28th, the pain in his side, his only complaint, was slight and not constant. He took half an ounce of the salts in the morning, and a small dose of James's powder, with two grains of calomel, at bed-time.

"On the 29th, he had no complaint whatever, except the distress arising from his body being wholly covered with the eruption, called in the West Indies the *Prickly heat.* That day and the 30th, he took the same medicines as on the 28th, and on the 31st returned to his work.

"On the 1st of November, about three o'clock P. M. after eating his dinner at noon, with an excellent appetite, he was suddenly seized with giddiness, severe headach, great thirst, and a burning heat of the surface; although, according to his own feelings, he was cold. Twenty ounces of blood were immediately taken from his arm; he then took an emetic, and after its operation, which soon occurred, he took twelve grains of calomel.

"He enjoyed some sleep in the night, and next morning the headach and thirst had greatly left him, but he had had no stool. On account of the state of his pulse, which was 80, full and strong, and of the redness and fulness of his eyes, twenty-five ounces of blood were again abstracted, and he took an ounce of salts. The same quantity of salts was repeated at noon, and half an ounce at four o'clock. By eight at night the salts had operated fully, and he declared himself free from any sort of pain, except a general soreness. He took six grains of James's powder and two grains of calomel at bed-time.

"On the 3d, the last medicine was repeated. On the 4th, he took fifteen grains of rhubarb and two of calomel, and was allowed half a pint of a weak white wine.

"On the morning of the 5th he complained of griping, and took an ounce of salts, which operated several times during the day; yet at noon he felt a return of the headach, which increased in severity till six in the evening, when his skin was hot and dry; his pulse full and firm; his tongue whitish, but he was free from thirst. Twenty-eight ounces of blood were taken from his arm, and his limbs were sponged with cold vinegar and water. At eight o'clock, the redness of his eyes, heat of his skin, pain in his head, and fulness of his pulse remaining unchanged, a vein was again opened, but only twelve ounces of blood could be procured. He then took five grains of the pulvis antimonialis. At 10 o'clock the medicine was repeated, and a blister placed on the nape of his neck.

"Although he slept very little in the night, and his skin remained perfectly dry, his head was considerably better next morning, when he was sent to the hospital at Port Royal."

This patient, notwithstanding the prodigious quantity of blood he had lost, was not a whole day confined to bed. He had most violent attacks of the headach during eight days that he remained at the hospital; and, after he returned to the ship, he was nearly a month subject to repeated returns of it, during the whole of which time his bowels were always inclined to costiveness, and were only moved by the constant use of purgative medicines. After having been about a week entirely free from any complaint, he was suddenly seized, on the night of the 23d of December, with a most severe attack of acute hepatitis, conjoined with pneumonia, for which he was twice bled, to the extent of fourteen ounces at one time, and of eighteen the next, and then sent to the hospital at Bermuda. After remaining there a month or six weeks, he was sent to England for the recovery of his health, although at that period free from any symptom of local disease, and likewise from any remarkable debility.

CASE V.

"Lieutenant R—— ætatis 30, was attacked the 5th of November, in the evening, with severe headach, restlessness, thirst, and universal uneasiness. Eighteen ounces of blood were taken from his arm, which almost instantly removed the headach. He afterwards took ten grains of calomel, which had a full effect as a purgative, and next day he felt no pain whatever. On the 7th, however, the symptoms recurred. His tongue was white, and bowels open. Twenty-five ounces of blood were taken from a vein, and he took ten grains of calomel, with the like quantity of jalap. His skin felt extremely hot and dry through the day. He was free from thirst and nausea. At four o'clock he took an ounce of Epsom salts, which was repeated at eight; and, at eleven, he took an ounce of the oleum ricini, which, being speedily followed by several loose and very abundant stools, greatly relieved his head.

"On the 8th he had no pain remaining, but his skin felt dry. He was directed to take a mixture containing the aqua acetitis ammoniæ, but finding it disagree with his stomach, he took but a few doses. On the 9th he began to take a drachm of the bark three or four times a day in water, and likewise two grains of calomel at bed-time. On the 14th he returned to his usual duty free from uneasiness, but the state of his bowels required some stimulus every other day for eight or ten days longer."

Case VI.

"Sunday, 6th November.—Samuel Hawkes, ætatis 28, blacksmith, was attacked in the afternoon with violent headach, sickness, and vomiting. He had been intoxicated the day before; was much addicted to that practice, and was a person liable to disorder of his intellectual powers, almost equivalent to mania, from such excess, which he accounted for when sober, by saying that his skull had once been fractured, marks of which injury are still evident.

"Twenty-four ounces of blood were taken from his arm, and he took an emetic, which operated well; after which he enjoyed some sleep, but perspired none whatever. Next morning the same quantity of blood was detracted, and he took twelve grains of calomel, with as much jalap. At six in the evening, although he had had several stools, the pain in his head continued very severe, yet he had no thirst. The blood-letting was repeated as before, and he took six grains of the pulvis antimonialis, with two of the submuriate of mercury at bed-time.

"On the 8th, The headach and giddiness continued, though rather less than before; his tongue was white; there was no appearance of perspiration, and his thirst had again become more pressing. In this state, he was sent to the hospital at Port Royal, along with several others affected with the same disease.

"He returned on board, in a state of convalescence, on the

13th of the month: resumed his usual occupation on the 21st, and has not since had any return of disease."

CASE VII.

"Tuesday, 22d November.—Mr N———, ætatis 30, was attacked soon after noon, with a general sense of cold, and with pain in his loins. At four o'clock, he took ten grains of calomel. At seven, he lost twenty ounces of blood, and took an ounce of sulphate of magnesia. At ten, the salts being repeated, caused instant vomiting.

"He had but one scanty stool in the night, and scarce slept any. At seven next morning, he was affected with severe acute pain in his head, especially betwixt the eyes. His pulse was 100, full and strong, his skin extremely hot and dry, and his tongue white and moist. The bleeding was repeated to twenty-eight ounces; he took five grains of calomel, and was directed to take every hour two drachms of sulphate of magnesia in half a wine glassful of water.

"By six in the evening, he had taken two ounces of the salts, and had had several small loose stools. The pain in his head, which had been somewhat relieved after the bleeding, had recurred in the afternoon as severe as ever. His thirst had become more urgent, his skin was still very hot and dry, and his pulse 100, strong and thrilling.

"Sixteen ounces of blood were now taken from his arm, a blister applied to the nape of his neck; he took at bed-time five grains of the pulvis antimonialis, with one of calomel, and had his barley-water acidulated with tamarinds.

"On the 23d, his pulse was 100, and soft, and though he had not perspired, his skin was cool, and his head greatly better. The blister had risen well, he had had several stools, and had enjoyed five or six hours sleep. He was now affected with strangury from the cantharides.

"He was directed to take a mixture, composed of the aqua acetitis ammoniæ, and to drink abundance of mild diluents.

"At noon, he was entirely free from uneasiness, his pulse 90, and weak, his skin quite cool. He expressed a desire for food, and took a little chicken-broth. He began to take half a drachm of the pulvis cinchonæ every three or four hours. In the evening, his limbs were bathed with vinegar and water, and he took five grains of the pilula colocynthidis composita, at bed-time.

"Next morning, he complained that the headach had recurred with great violence about an hour after going to bed, but said that it had not lasted above an hour. Still, however, he felt an oppression about the eyes, his tongue was white, and his pulse 90, and weak. He had a copious alvine evacuation, was quite free from the strangury, and had a desire for food.

"He was directed to take a drachm of the bark, with fifteen drops of diluted sulphuric acid, three or four times a-day, and was allowed part of a fowl, and half a pint of wine.

"At bed-time, he felt slightly giddy on moving quickly. He took two grains of calomel.

"On the 25th, although he had slept much and soundly, he felt languid and giddy. He took two drachms of sulphate of magnesia, and the wine was omitted.

"Towards the evening, the headach became again troublesome, although the salts had procured several stools. On attempting to walk, he was suddenly seized with giddiness and vomiting. He slept a good deal, however, in the night, but awoke on the morning of the 26th with a severe headach, his tongue covered with a white fur, and his pulse at 88 in a minute. He felt extremely weak and languid, and vomited every thing he took. On attempting to sit up for a few minutes, spontaneous vomiting came on. At noon, he took a cupful of thin arrow-root, which his stomach retained. At one o'clock, a glyster, containing an ounce of Epsom salts and an ounce of olive oil, being thrown into the rectum, was soon followed by several fetid, dark-coloured stools, in which there floated white fibry-looking substances. About three o'clock, the pain in his head lodged particularly over the left eye; his pulse was 88, and firm. The temporal artery, in particular, pulsated visibly. I therefore punctured a branch of it with a lancet, and drew off, by means of cups, fully four ounces of blood, which gave considerable relief. The anterior half of the hairy scalp had been previously shaved, and was kept constantly wet and cool with vinegar and water. After the cupping, he took three grains of calomel, and his stomach retained arrow-root, of which he took a little every two hours.

"At eight in the evening, after the operation of a second glyster, he felt his head much relieved, and he was free from the extreme faintness and languor which had distressed him in the morning. His pulse was still at 88, the temperature of his skin was 100° per thermometer, yet he complained of cold. He felt a numbness in all his fingers. At bed-time, he took seven grains of calomel. The arrow-root was given in small quantities every two hours during the night, and his limbs were several times sponged with vinegar and water before midnight, when he was covered with a moderate perspiration. At that hour, his pulse was 84, and regular; and though his breathing was occasionally hurried, the headach gradually became less severe.

"On the morning of the 27th, his pulse had fallen to 76 beats in a minute, and he complained only of slight faintness in sitting upright.

"He took four grains of calomel, which procured several green-coloured, fluid evacuations. He had no return of fever at night, and took ten grains of the pilula colocynthidis composita.

"On the 28th, in the morning, he took five grains of the same pill. Throughout the day, he felt lively and free from pain, took his food with appetite, and one glassful of wine. In the afternoon, he had several abundant, fetid, greenish stools, passed with tenesmus. At bed-time, he took five grains of the pilula hydrargyri.

"After going to bed, he became restless and unable to sleep, though free from pain and from fever, his pulse being 64, and regular. Under these circumstances, I was tempted to give him one grain and a half of opium; but, instead of procuring sleep, it caused more restlessness, and next morning he was afflicted with headach, his tongue was extremely white, and he rejected, by vomiting, some tea he had taken for breakfast. His skin, however, continued cool, and his pulse at 64.

"At eight o'clock, he took three grains of calomel. At one P. M. a glyster, containing two ounces of Glauber's salts and one of olive oil, being injected, was retained an hour and a half, and then brought away a quantity of dark green feces. At six in the evening, the headach, which had been chiefly occasioned by the opium, entirely left him, and after that day his convalescence was established, although he required for several weeks occasional, indeed almost daily, doses of purgatives; for, whenever he neglected his bowels, he had severe returns of the headach, and has ever since been affected with unusual sensations and weakness in his left eye."

CASE VIII.

"Mr E., aged about 23.

"Nov. 23, 1814.—Was affected with severe headach, chiefly betwixt the eyes, which were full and watery. He took six grains of calomel and twelve of jalap, in the morning, and at noon an ounce of sulphate of magnesia, after which he had several loose stools. At night, 16 ounces of blood were drawn from a vein in his arm, his limbs were bathed in warm vinegar and water, and, at bed-time, he took six grains of the pulvis antimonialis, with two of calomel.

"He slept very little, and scarce perspired any in the night; and, next morning, the headach continued severe, his pulse was at 90, and full, his tongue white, and skin moderately hot. Thirty ounces of blood were taken from his arm, and he took an infusion of two drachms of senna, with one drachm of cream of tartar and a proportion of sugar, in solution, which soon operated fully on the bowels. At eight o'clock that evening, he was free from any sort of uneasiness, and from pyrexia. His pulse was at 80, and his skin cool, but dry. He took 25 drops of the liquor ant. tart. of the London Pharmacopœia, at bed-time.

"He was excessively restless in the night; the medicine caused nausea, without inducing perspiration. In the morning of the 25th, his pulse was 90, and thrilling, and the pain across his forehead very intense. Twenty ounces of blood were taken from a vein, and the infusion of senna repeated. In the afternoon he felt much better, and ate some food with a surprising appetite against remonstrances, and, at bed-time, took ten grains of the pil. colocynth. comp.

"He awoke from a sound sleep on the 26th, with a violent headach, and with his pulse at 90. He immediately took five grains of calomel and ten of jalap at noon, half an ounce of the sulphate of magnesia, and, at five o'clock, half an ounce of castor oil. These had a full effect at seven o'clock, and he felt immediate relief.

"On the morning of the 27th, his headach continued severe, his pulse 84 and full, his skin hot. He took six drachms of Epsom salts, kept cold fomentations of vinegar and water constantly to his forehead, and, at one o'clock afternoon, took half an ounce of the oleum ricini. The headach continued violent, his pulse quick, and his skin burning hot, till the evening, when his bowels being fully purged, the pain and fever abated. He took fifteen grains of the compound colocynth pill at bed-time.

"He had a good night, and the following morning was free from pain, his pulse 68, and steady; tongue slightly furred near the root, and his bowels open. He took one ounce of the sulphate of magnesia, and, at bed-time, one grain of calomel.

"It is unnecessary to detail any further the particular treatment of this case. It is sufficient to say that, for two or three weeks, dose after dose of one purgative or other, once, and sometimes twice a-day, was absolutely necessary, on account of the returns of headach, and its concomitant costiveness. In the meantime, the bark was taken in moderate quantity, and the patient finally became quite well, without having been a whole day confined to bed."

However interesting these cases may be, they are not more conclusive in favour of free and extensive blood-letting in this fever, than many others which I could easily adduce, even from my own short experience; but these are sufficient to prove that it is erroneous to consider the yellow-fever in any other light than that of a violent inflammatory disease. Certain I am, that whoever goes to the West Indies, and persists in treating it as he would typhus, will have frequent reason to lament the fatal effects of such practice.

His Majesty's ship Erne, 21st April 1815.

XVI.

On Pathology.

DISORDERED actions in the human body are, generally speaking, the means which Nature employs for the expulsion or removal of offending agents; thus, if the stomach be excited to vomit, the cause producing the disturbance is removed by that action. If any extraneous substance irritate the membrane of the nose, the diaphragm, by its associating nerve, is thrown into action, and sneezing expels the substance; thus also diarrhoeas carry off noxious matters; and the emunctories of the body are generally cleared out for the same purpose.

The human frame reacts against mechanical as well as chemical agents; thus a piece of hard substance excites the secretion of tears when it is introduced into the eyes, which tears wash out the substance; and sapid bodies excite the saliva, which dilutes the solid ingredient. A calculus passing the ureter excites vomiting; and a hernia is drawn back by a countervailing retraction of the omentum by the stomach. A gall-stone excites vomiting, and the bile by that process is forced into the duodenum, thereby carrying before it the concretion.

Parts are also adapted to the proportion of effect that is to be produced, and an almost involuntary resistance is given against the introduction of noxious agents. The most tender organs are protected with the greatest care, and those most essential to the existence of the body are placed in parts most secure from accident. Innumerable instances may be found in physiological works, which tend to demonstrate that provision is made in every part of the human frame for the preservation of the body.

Bichat remarks, that " life consists in the sum of the functions by which death is resisted." As a machine the human body may be said to *go*, at the same time that it includes powers for repairing all injuries that otherwise would prevent its going, and these reparatory processes include almost all the symptoms of disease. If a substance be taken into the stomach which is not nutritious, or not consistent with the well-being of the frame, a vomiting may expel that substance,—this vomiting would be salutary, as it would prevent the evils that might otherwise disturb the body. If the stomach did not reject this noxious substance, and it were to pass on into the alimentary canal; and if the liver were to pour forth an unusual quantity of bile, and that bile were to occasion a diarrhœa, and by such means the noxious substance were expelled,—such a derangement of the liver would be considered salutary, and that increased quantity of bile could not fairly be regarded as disease. The reactions of the stomach and of the liver may therefore be, in some instances, conceived as efforts beneficially excited to remove noxious agents.

Should this salutary reaction of the stomach be kept down, and should means be employed for the prevention of that sickness which would, without such interference, have removed the offensive agent, that agent must still remain there, and farther and more extended reactions must be excited for its removal by other organs of the body, and thus it happens that the liver takes up the business, and succeeds in expelling it through the bowels. It will be shewn that, where the reactions of the stomach are checked and prevented, that severe bilious affections are often the result, and this result generally arises where liquors of a temperate quality, and which excite stomach reactions, are superseded by more ardent spirits, which keep down these salutary reactions. Thus dram-drinking disorders the liver, and the milder liquors the stomach. We may fancy that, in a state of nature, these reactions are positively curative, but that, in as far as we are perpetually calling forth the necessity of these powers, they must become less able to contend with the evils. Thus a long series of disorders, called forth by the repetition of noxious causes, must occasion an incompleteness in the resisting means, and thus must other organs be concerned in the curative process. We will suppose, for example, that the stomach, unable to perform healthy digestion, presents to the liver, as it passes the duodenum, an ill-concocted chyme or chyle, does it not become necessary that the liver should pour forth a bile suited to the purpose it had to answer? a purpose far different from what would be required if a healthful digestion had taken place in the stomach. Such a bile cannot be deemed improper, since it answers the purpose for which it was intended, namely, of carrying through the bowels what was noxious, and of effectually assisting in assimilating such parts as are healthful and proper. To attack the liver, therefore, because it has done its duty, would be adding to the evils which had already excited its powers, and would be exhausting those means of resistance and reaction, which were appointed for the most beneficent of purposes. To oblige, by medicine, the stomach to retain such substances, as in a state undisturbed by medicine it would reject, is the readiest conceivable method for calling forth the symptoms of liver affection, and a general disturbance of the alimentary functions; and thus it happens that the more extended reactions of the constitution follow these circumstances, and thus, by a very easy process of reasoning, shall we arrive at those causes which produce gout, asthma, cutaneous disorders, and in short a long train of grievous maladies. During healthful digestion, feelings are excited far different from those which arise when the meal has not been regulated by moderation and sobriety; and how often are means applied to appease the tumult occasioned at such times, and thus so many noxious agents are introduced which become the causes of great and extended future mischief.

The disorders of the digestive organs have been deemed the cause of most diseases, constitutional as well as local.—To me they appear to be the cure of such extended operations. We cure distant affections by such means as disorder the digestive functions. We disturb the stomach, and we agitate the bowels, and by so doing we remove constitutional fevers, and check the progress of local excitement. Reactions become more general when the alimentary process makes no resistance, and local affections enhance the powers of the stomach in vicious indulgence. In this manner, the gout allows the stomach a more extended range, and with this painful appendage is a person capable of digesting a larger mass of otherwise indigestible substances.—Though the stomach may be offended with table beer, the gout is nourished by Madeira, and I think the deep-seated reaction of gout is far less desirable than the curative, though troublesome, reactions of the stomach. It is a dear purchase of ease, particularly when moderation and sobriety are sacrificed. Those persons who feel the most troublesome disturbance in the stomach after a debauch, are not ultimately so much injured as those who carry off without inconvenience the same causes of evil; and those liquors which keep down and deaden, as it were, the reactions of the stomach, are those which occasion the deepest and most serious maladies. The disorders of the stomach, the deranged actions of the liver, and, in short, the common and troublesome disturbance of the alimentary functions, are diseases more in idea than reality, and I shall attempt to shew, in a future work, which will contain the detail of the system which these outlines embrace, that the practice of medicine, which implies an attempt to remove these affections, has immeasureably increased the evils these reactions were naturally ordained to remove. The practice of medicine should be directed to assist and not subvert these natural operations; and in so doing we should be guided by a strict observance of natural laws.

It has been said that foulness of the bowels is a common cause of disease. It appears to me that when the bowels produce the foulness so often observed, such foulness proves curative. It is a reaction of the liver against a constitutional disturbance, which in the end proves curative. Immediately on discovering this foulness, we feel satisfied that on its removal the various symptoms of disease will disappear. I should contend that the process causing foulness proved curative, and that the liver took, as it were, upon itself the management of the cure. There is a balance in the constitution consistent with every natural effort, it may be called the diathesis, such as of gout, and a variety of other inflammatory affections, and these states of balance involve their own series of phenomena. Thus the head may be oppressed with a superabundance of blood, and be liable to affections under one form or series; another may involve rheumatism, another gout, and another that cause which produces foulness of the bowels; all of them extended reactions of the system, tending towards a reparation of the constitution. It may be observed, that foulness of the bowels cannot exist to the full extent at which it appears at any one period; for the quantity that on some occasions is discharged, would be more than the canal was capable of containing. It must be the result of successive depositions from some great secreting organs; for instance, during the existence of diseases wherein there are great determinations of blood, whilst the balance of symptoms involves the inflammatory states of those disorders, aperient medicines bring away evacuations of no particular character; but after a time the circumstances of the case alter,—heavy, lumpy, and discoloured evacuations begin to appear, and by the aid of very gentle means, continue to be parted with. As soon as these appearances arise, the symptoms of the original disorder begin to diminish, and in the course of a short time disappear altogether. It must have occurred to every practitioner who has strictly examined these circumstances, that he has found a difficulty in accounting for the quantity and extent of this collection of foulness. It must also have been frequently observed, that affections of the head, epilepsy, chorea, local diseases of various kinds, and great and extended affections of the skin, have all given way as soon as the bowels have expelled a quantity of foul and fetid evacuation. During the progress, however, of these maladies, the bowels have not shewn the same character until the disorders have attained a particular stage, and then the progress towards health is decided. Could we succeed in bringing about this stage, many very grievous maladies might be cured; that is, we might induce thereby the liver and other organs of the alimentary canal, to render the more extended reaction of the system unnecessary. I do not mean to deny that there is occasionally a very great accumulation in the bowels, so foul a state of them that worms occur, which appear therein to have found a proper nidus; and that other great sympathetic affections take place arising from this accumulation. If we trace these affections, we shall find many natural efforts made to remove such accumulation and foulness; and even that many very distant reactions occur, tending to relieve the body of the grievance. Thus the blood, returning through the vena portæ, is delayed, and as the heart acts uniformly, more blood flows to the head than usual, in consequence of this remora in the return of blood from the lower circulation. This fulness of blood in the head, besides giving due support to the required and additional nervous activity, occasions many reactions, amongst which we may rank epilepsy, which shakes the whole body in convulsions, and is the means of removing worms and other foulness in the bowels; as under the influence of that disorder the alvine and urinary secretions are violently expelled. We shall see hereafter how nervous affections tend to the reparation of the body, and how far the mind is concerned in many processes which naturally obtain in the removal of noxious agencies.

Health may be defined to be that state of the animal economy where there is no cause existing to interrupt or disturb a certain balance of natural actions. Whenever any part of the human body is interrupted in its natural functions by any offensive agent, certain constitutional powers are called into action to effect its removal. The exertion of these constitutional powers is to be considered as a curative process adopted by nature to liberate herself from the offending cause. These powers, the result of a reaction in that part of the system which is disturbed, have been supposed to be the symptoms of disease; hence the attention of the practitioner has been directed to the removal of the reactions rather than to the dislodgement of the cause. How can a cause producing efforts of resistance, or the reacting efforts themselves be called disease? Neither the one nor the other comes under the strict meaning of the term. We may perhaps define disease to be that state of the human constitution, wherein the natural reactions against offending agents are not sufficiently powerful to avert death; and according to this idea the following principles may be laid down:—

1.—That there are some causes, among which the most obvious are the external, capable of producing disease.

2.—That the tendency of disease is either to spontaneous cure, or to the extinction of life.

3.—That the organs of an animal stand in various relations with each other, that the medium of their relation consists generally, if not always, of a nervous connection.

4.—That these organs influence each other in disease, or that the state of disease which one organ has assumed may influence the condition of another, although no very intimate connexion should be exhibited between them in health.

5.—That when disease takes place in one organ, or in one part, the influence which such a state of disease has upon another organ, or another part, is to produce a change in its function, which change, though furnishing an additional symptom of disease, is nevertheless curative, in regard to that which occupied the original seat.

6.—That this case holds good only to a certain extent; or more definitely that the success of the curative processes is dependent;—1st, upon the force and nature of the cause,—2dly, upon the degree of the disease,—and 3dly, on the organ in which it takes place.—Thus,

7.—A determination of blood to the brain produces disorder of the head; a rupture on its surface may produce hemiplegia, and a rupture of a blood-vessel near the origin of the eighth pair of nerves would produce immediate death.

8.—That in disorders not incompatible with the continuance of life, a secondary disease, which succeeds as a consequence of a primary one, is remedial in regard to its own cause—that if a third, a fourth, or a fifth function is disordered, the general tendency of these consecutive affections is to cure the primary one; and that to this end they all concur.

9.—That just so many functions undergo a secondary derangement as are requisite for the cure of the primary one.

10.—That if death happen in the course of the processes, whose effect would otherwise be remedial, the event takes place from a deficiency of vital energies to sustain the actions which are preparatory to the cure.

11.—That these tendencies to secondary disease are a provision against the effects of unnatural living; and are possessed, though rarely exercised by animals whose appetites are purely instinctive. That in such, the primary disease rarely takes place; and that in them, the same mode of cure is often exhibited when the primary does occur as an effect of domestication.

It has been remarked that the human stomach is an organ endued by nature with the most complex properties of any in the body; and forming a center of sympathy between our corporeal and mental parts, of more exquisite qualifications than even the brain itself. Yet the knife and eye of the anatomist do not discover the whole important station it holds in the economy; we must look to the living system for those nice connexions of cause and effect, and that source of association which give it a relationship to so many organs, both in the healthy and diseased state. An organ intended for such important purposes in the animal economy, must receive from the hand of nature singular tokens of her favour. Hence we find all those viscera, which assist in preparing the chyle, and what is called the assimilation of the food, joined in a circle of nervous communication, of which the stomach is the centre. One portion of nerves is distributed over the whole; so that, while they are employed in one purpose, disorder cannot take place in any of them without the whole being thrown into confusion. It would betray a great want of confidence in the acknowledged order of the laws of nature, to suppose that all these associated organs were not regulated in their apparently disturbed state, by laws tending to the relief of that perturbation, as well as for the continuance of their more quiet actions. It is, I contend, by these associated powers that the causes of perturbation are removed, and that the effects of such reactions are eventually rendered harmless. The nerves of the stomach are derived from the *par vagum*, or eighth pair, which communicates with the great intercostal or sympathetic, and by it is connected with almost every other nerve in the body. The semilunar ganglion of the great sympathetic, supplies particularly the liver, gall-bladder and ducts; duodenum, pancreas, spleen, jejunum, ileum, and part of the colon, &c. The renal glands, kidneys, ureters and bladder; the uterus, ovaria, testes, &c. are all supplied by the same nerve, and joined by others from the lumbar vertebræ. The muscles of the pharynx and trachea, those of the neck and lower extremities, are even connected by branches of this nerve. The lungs, heart and diaphragm, being all furnished with nerves which communicate with the great sympathetic, it would appear, that this nerve is the grand link or chain which connects the vital, animal, and natural functions with one another.

It would be no very difficult matter to trace the curative actions that take place in consequence of this nervous connection; how the mind and body vary in their proportionate power according as the exigencies of the constitution may require; how the heart may vary its pulsations agreeably to the impulses it receives through this nerve; how the liver, pancreas, and intestines generally are apprized of the necessities of varied exertions, agreeably to the kind of digestion that is to pass the sphere of their duties; how the kidneys become respondent to the impressions on the stomach, &c.; all, I say, for the beneficent purpose of ultimately removing from the system the noxious influence of offending agents; such influence as, were it not for these wise provisions of nature, would prove destructive to the human frame. These several reactions of the body seem all calculated to become effectual when the system is in a state agreeable to the laws of nature. A really curative process may be so far altered in its ultimate result, by improper habits of life, that it may not be enabled to answer the purpose intended, or it may run into an excess, and even occasion detriment to the subject.

The control over the human mind seems to depend upon real animal power, as much as those functions which may be strictly considered purely animal. During sleep, when the associations of the mind proceed regularly, uninfluenced by the control of the will, the animal powers are refreshed, and the mind awakes with repaired energies. Were it not for this salutary quiescence of the mind, the powers of the human body would be quickly exhausted, and its functions would be in a state of inaction. During the absence, therefore, of that power, which allows a control of the will over the human mind, the animal functions are repaired and restored. It appears that the healthy state of mind and body obtains when the power of the general constitution is complete. It also appears, that the mind and body reciprocally assist each other, that bodily enervation often enhances the powers of the mind, and that the mind is suspended in a determinate manner to allow a reserve of power to be employed in the reparation and restoration of the animal functions. Thus we see that, when the mind is unsettled in delirium, the bodily powers are much improved, and that the strength of the body wonderfully increases as the power over the mind diminishes. This fact could be demonstrated in numberless instances, where the power of the will, and the control over the mind, yield to great animal passions and exertions. If, during the dereliction of the mind, the constitution is strengthened, and a reparation of the body takes place, the delirium of fever, as well as many other aberrations of the mind, may be considered as salutary and sanative. The mind and body alternately supply each other, accordingly as either may be required in the progress of a cure. Thus we see that some great passions of the mind remove general constitutional affections, and great bodily power obtains, when the mind is unsettled and disturbed. Many of the animal powers are wonderfully increased when the mind is unsettled, and great excitements of the mind keep off and restrain many constitutional reactions. The habitudes of the mind influence the functions of the body, and, like other curative reactions, become the causes of extended recoveries of local and constitutional maladies. We know that men who, in the quietness of peace, would lose their lives by exposure to the inclemencies of weather, bear, when roused by active service, the greatest hardships and privations. We see that many of the most formidable diseases give way, or are arrested in their progress, as soon as the mind is not under the guidance of reason, and that during frenzy, wounds heal and disorders disappear, which threatened, whilst the mind remained sound, the most calamitous consequences. I have seen maniacal symptoms, supervening on dropsy, prove critical of that disorder, and life prolonged to an extent hardly conceivable, when mania has occurred in the most threatening cases. It must, in short, be acknowledged by every practitioner, that such circumstances are of very frequent occurrence. When the mind is any way affected, I conceive, that then the power which was employed, and which was necessary to the preservation of the due reasoning faculties, is engaged in another direction, for the purpose of the reparation of the body, and for the restoration of it to that state where again the mind may enjoy its full powers, and be again restored to the full possession of its functions. We will suppose that the brain, like the metropolis, is the seat of government, and that the nerves, &c. make a communication from the most distant parts of the empire, and that, throughout the system, provision is made against rebellion, or against the introduction of a foreign foe. We will imagine an enemy to land on a distant shore, and that the local reaction repels his efforts; no great constitutional disturbance takes place since the local power is sufficient for the purpose. If this enemy should overcome the local force, and should advance, a more determined resistance and opposition become necessary; and many contiguous parts unite for the purpose of expelling him. Should, however, the enemy prevail, and should he advance still farther, the government of the country may be obliged to quit the capital, may for a time abandon its common cares, and direct its attention solely to the instant necessities. All its energies may be exerted to overcome the enemy, and when this is effected, it resumes its seat, and its functions go on in the usual manner. Many sacrifices are however often necessary after such a conflict, and many parts of the empire feel for a long time the effects of such a mischief. This appears to be the case with mind and body, and thus is the absence of the higher qualifications of intellect only proportioned to the exigences in the animal frame; and when that power, which under other circumstances gives force to the reasoning faculties, has served the purpose of repairing and invigorating the functions of the body, does it return to its accustomed seat again to regulate the whole constitution of mind and body. This analogy might be carried farther, and a number of instances of fever, and a long list of mental diseases, might be produced to show the general conformity; but it is sufficient for the present purpose to mention general principles, as the details are immediately obvious. Whenever, therefore, we see nervous complaints, wanderings of the mind, and the whole list of disorders that involve any alienation of the reasoning faculties, we may conclude that the power is elsewhere employed, and that the tendency of such alienation is for the restoration and preservation of the whole animal system. It is thus that the human species remains nearly the same for so many ages, for if we were not to believe that the errors of mankind were corrected after some few generations, the whole race must have been long since extinct. Predispositions to disease may in this manner be supposed to be ultimately corrective of evil. Vicious indulgence of all kinds produces pains and evils which extend their baneful influence through many generations, but ultimately it appears that such evils prove corrective, and that at the present day there are as healthy children born as there were in the remotest periods of antiquity. If it be admitted that the symptoms of disease may be resolved into a process reparatory of the human frame, and if the alienation of the human mind allow an increased power to be employed in the restoration of the constitution, it appears to me that such provisions are made for the ultimate protection of the human race.

Such medicines as call forth the reaction of the stomach are found useful in the more extended disorders of the constitution; and thus does it at first sight appear that many of the most important diseases arise from a derangement of the digestive organs. If the powers of the stomach had been allowed to have been exerted in an effectual reaction against the early introduction of noxious agents, and had those reactive powers been attended to, as indicating the baneful consequences of a continuance of bad habits, many very grievous maladies would have been prevented. Perhaps it is only by retracing the steps of such disorders through the organs originally affected that any effectual relief can be obtained, by avoiding such habits as originally called forth the reactions of the stomach, and by forcing the stomach and all other intermediate organs to react against those causes which have proceeded to a greater extent in the constitution. It is thus that operations on the stomach, liver, and bowels, are so effectual in removing many great and extended disorders of the system. Nervous disorders, and all those mental diseases which imply extended reparative provisions, are all influenced under their respective circumstances by such means as call forth reactive exertions in the primæ viæ. So that whether a disorder originates in the stomach, proceeds to distant organs through the stomach, or is a disease locally inflicted or arising primarily in any distant part of the frame; still such reactions are capable of affording relief. For as the stomach is of the greatest importance in receiving and assimilating the nutriment which is absolutely requisite to the remotest part of the human fabric, so is it at all times equally important in regulating a digestion adapted, under all circumstances, to the necessities of the case, and in all those reactions which prove curative of the evils that assail the constitution in any part. Much caution is requisite in ascertaining the stability of the frame on which we act, before we venture to alter the plans already adopted by nature. One state of body as to strength implies one series of phenomena, and disorders often naturally supersede each other, agreeably to the compatibilities of each with that strength; and thus we often see hæmorrhagies, cutaneous eruptions, erysipelas, a variety of local inflammations, affections of the head, gout, &c. succeed one another under every modification, agreeably to the compatibility that the state of the system holds to each individual disorder. The changes in complaints and the succession of symptoms are thus to be accounted for when the detail of this system is explained.

It appears to me to be demonstrably true, that we might as correctly conceive any irregularities of the heavenly bodies, though producing in every direction complete correction in their ultimate motions, to be diseases and disorders of the Solar System, as that the corrective laws which regulate the animal economy should be considered as such. The wise Author of the universe has established laws which promote, support, and correct where irregularity occurs, the system of the human frame, as well as the system of the world generally, and these laws act to the preordained extent and duration of the subject. *Thus*, although all men must die, it does not follow that thousands and millions should meet a premature grave, or that the short period of life which is allotted to so large a portion of mankind should be marked by pangs and anguish, which have no beneficent purpose, or which could be followed by no ulterior advantage. The pains which follow in the train of debauchery, or any unnatural modes of life, may prove morally corrective of the errors that occasion them, and it is in the physical irregularities that are thus produced, that I contend there is a reparation of the physical frame. If these correctives are not attended to, if the causes are continued which produced them, a period must arrive to the resisting powers of nature, the frame must be destroyed, and organic injuries must arise, that quickly subvert the constitution. How could mankind live in the most variable climates, or digest food of every sort and difference, did not the original laws accommodate themselves to such particular circumstances; and how could the frame exist, were it not for these wise laws of nature; when under every kind of disease, and under every diversity of circumstance, the same stomach acts and the same organs produce their respective functions agreeably to these altered necessities?

I intend, on some future occasion, to trace the natural order of phenomena which diseases present in the regular series of their respective symptoms, and to suggest a practice of medicine resulting from the above principles and modes of reasoning.

MEDICUS.

PART II.

CRITICAL ANALYSIS.

I.

Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Volume the Fifth. 8vo. pp. 456, with eight Engravings. Longman and Co.

We are now called upon annually to notice the useful labours of this active and flourishing Society, which has already accumulated a considerable mass of professional information, to which this new volume will be deemed an important addition. The following statement in the advertisement prefixed to this volume, while it evinces the zeal of the Society, opens a promise of some valuable communications from a new source. It is affirmed, that the correspondence of the Society has increased in extent and importance; and that "one of the great departments of the state has, with a degree of liberality and public spirit, which the President and Council have great pleasure in acknowledging, imparted to the Society, for the purpose of publication, various important documents relative to diseases in the army;" a mark of confidence, which equally redounds to the honour of the Society, and the advantage of the public.

It would be difficult, in the limits of a single article, to give any satisfactory analysis of the contents of twenty-six papers, which this volume contains; and this, indeed, is the less necessary, since the work is becoming a standard of reference, which the majority of literary practitioners will place upon their shelves, as a continuation of the Edinburgh Medical Essays, the Medical Observations and Inquiries, and other collections of established reputation. We shall therefore content ourselves with a brief enumeration of the subjects of the respective communications, without entering generally into any minute details of the facts related, or of the opinions maintained.

The *first* paper contains an ample history of a case of "periodical jactitation," of a very extraordinary nature, which occurred in a young girl, and after continuing several months, appeared to terminate in consequence of the occurrence of a spontaneous diarrhœa, which was encouraged by purgatives. The paroxysms of furious jactitation were regularly periodical, and consisted of rolling, whirling upon the feet, or flinging up the body and lower extremities, &c. with loss of speech and of the power of walking, but without insensibility. Some other cases of this kind are alluded to, and it is stated, that so late as the year 1697, seven persons were burnt by the verdict of a jury at Paisley, upon the charge of witchcraft, supposed to be practised in a similar instance.

The *second* paper contains a brief account of the symptoms and appearances on dissection, connected with an abscess in the brain, which occurred twelve months after the infliction of a blow, which at the time occasioned very little inconvenience: it is from the pen of Dr Denmark, of Haslar Hospital. That which follows, is a systematic essay on "night-blindness," or *nyctalopia*, which the author, Mr Bampfield, a navy surgeon, calls "*hemeralopia*." Hippocrates indeed, as has been often remarked, appears to use the term in the sense adopted by the author; but Galen, in the Isagoge, Paul of Ægina, Œtius, Pliny, &c. call the disease nyctalopia, and are followed by Hillary, Heberden, and other learned physicians among the moderns. The author has seen above three hundred cases of the disease, in hot climates; and though he acknowledges his inability to explain the cause of the complaint, he affirms that a succession of blisters on the temples has never failed to remove idiopathic night-blindness. In those cases in which it is connected with scorbutic symptoms, these must be previously corrected. Various remedies, recommended by authors, are noticed; and, on the whole, this paper comprehends the most complete account of this disease which we have seen.

The *fourth* and *fifth* papers contain two cases, in which cartilaginous substances were extracted from the large joints; in the former, from the knee, by Dr Clark, surgeon to the forces; and, in the latter, from the elbow, by Mr Coley of Bridgworth. The *sixth* is "an account of a chemical examination of the urine and serum of the blood of a person, who had been taking large quantities of soda," by Dr Bostock. The patient had been taking two ounces and a half of subcarbonate of soda daily, for a long time, during which period Dr Bostock's experiments were made. The details of the analyses evince all the accuracy, perspicuity and simplicity, by which this able chemist has been long distinguished. They prove that both the urine and serum of this individual contained a quantity of uncombined soda, which was attached, in the former, to the urea. Notwithstanding the alkaline nature of the latter, the blood was in a highly inflamed state, the crassamentum being much cupped and buffed. The *seventh* paper consists of a brief history of the symptoms and appearances on dissection, observed by Mr Chevalier, in a boy of fourteen, who died on the second day after a violent vomiting, by which the internal coat of the stomach and duodenum had been extensively lacerated.

A short, but valuable communication from Mr Earle, occupies the *eighth* place in this volume, and merits the attention of surgeons. It relates to the method of treating those contractions, which occur after burns or extensive ulcerations, and details the circumstances of a case, in which, upon scientific principles, a new and successful operation was performed. This is followed by a singular "history of a child retained in the mother fifty-two years after the usual period of utero-gestation," ably detailed by that distinguished pathologist, Dr Cheston, of Gloucester. This history, though somewhat analogous to the extra-uterine cases, differs from them in many essential points.

> "It exhibits a fœtus, after arriving in the uterus from the seat of its original formation, remaining nine months, and acquiring the full size in that organ, on the point of being expelled from its cavity by the process of parturition, when it escapes, in an almost incomprehensible manner, from the uterus, and becomes surrounded by a bony covering, in which it is found at the end of fifty-two years, almost as perfect in its form, as we should have expected to find it, if it had been born at the natural period of utero-gestation." (p. 105.)

The clear descriptions and intelligent observations of this paper give it considerable interest: and it is farther illustrated by three engravings, containing five representations of the bony tumour and its contents.

In a communication, which follows, though neither very novel nor profound, Mr Wardrop has given "an account of some diseases of the toes and fingers, with observations on their treatment," which will be useful, because it regards some little maladies, which are generally neglected as unimportant. These are inflammation of the soft parts surrounding the nail of the toes, the onychia maligna, corns, and chilblain. The *eleventh* paper contains some observations by Dr D. Stewart, on the causes which occasion abortion, by destroying vitality of the fœtus *in utero;* but is principally occupied with the history of eight successive pregnancies and abortions, at the period of seven months, in a lady who was delivered of a healthy boy in her ninth pregnancy; in consequence of the use of suppositories of opium, which were recommended by Mr Croft. The death and premature delivery of the children had always been preceded by acute pains in the abdomen, with extreme irritability of the bowels, constant diarrhœa, and tenesmus; but these were completely removed by the occasional introduction of two or three grains of opium into the rectum, and the ninth child was born alive and healthy. Upon very slight and vulgar grounds, Dr Stewart suspected the father to have a venereal taint, merely because he had an obstinate sore on one of his ankles; and, in her seventh pregnancy, he made the lady's mouth sore with mercury; but the child was again born dead as before.

Mr Wilson, of Stewarton, has communicated a case of that formidable disease, the cynanche laryngea, which terminated favourably, after one copious blood-letting, nearly *ad deliquium*, on the second day. The symptoms were well marked as arising from inflammation of the epiglottis and rima glotiddis, the former of which was discovered, by strongly pressing down the tongue, "enlarged to nearly the size of a small plum, red and glossy." Blistering and purging were combined with the free venesection. The combination of difficulty of swallowing with dyspnœa, the suppressed voice, the absence of the ringing-cough, the author observes, mark the difference between this disease and croup, which is besides principally a disease of children.

The *thirteenth* article is an elaborate and interesting view of the principal *monstrosities* of the human body, by Mr Lawrence, containing remarks on their production, and a statement of some physiological inferences to which they lead. These are preceded by an account of an acephalous fœtus, which, notwithstanding the deficiency of brain and cranium, lived four days, being perfectly formed in all its other parts, and having attained its full size. In this respect, Mr Lawrence observes, the acephalous productions are an exception to the general rule, that, where any of the principal organs are defective, there is always great confusion in the subordinate parts.

Mr Lawrence first enumerates the mere *varieties* and *lusus naturæ*, which are observed in various parts, without any material derangement of the functions, in the muscles, blood-vessels, skin, viscera, &c.; and afterwards those which are considered as *monstrous*; and then proceeds to describe methodically those imperfect organizations of a more striking kind, which come under the latter denomination. These are detailed under four heads, unnatural formation, unusual position, deficiency, or redundance, of different parts; and the account is particularly full with respect to the last, or supernumerary parts, including the many varieties of double and united fœtuses, and those inclosed in the bodies of others. In his remarks, which follow, Mr Lawrence has entered into the question of the pretended influence of the mind of the mother in producing these varieties and monstrous deviations in the structure of the child within her; a task which we should have hoped might have been superfluous in these times, but that we have heard even persons within the pale of the profession maintaining this vulgar nonsense; and that the majority of unprofessional persons hold it as a creed, which, like some other creeds, indeed, they have adopted from habit, and not from any rational investigation of their solidity, but to which they therefore adhere the more firmly. Mr Lawrence has refuted the absurdity to absolute demonstration. (p. 201-207.) From respect to the name of Haller, he also refutes at some length the notion, that these monstrosities can be produced in any case by *mechanical* violence; a proposition which is manifestly untenable: and he then proceeds to shew the probable sources from which they originate, and the inferences to which they lead. These monstrosities are perhaps exclusively found in domesticated animals; and domestication is not only the prolific source of morbid changes in all the organs and functions of the living body, but of all the infinite diversities of form, size, and colour, in tamed animals, some of which cannot now be referred to their original wild species. The function of generation is subject to the same disturbing causes: and these monstrous aberrations are, therefore, as Mr Lawrence contends, upon the same footing with unhealthy executions of the nutritive, secretory, and exhalent functions, and form no exceptions to the common rules of operation in organized beings.

The inferences deducible from these monstrous productions are beneficial to physiology, by shewing us the comparative importance, and the actual agency of particular organs, which are now and then found deficient. They shew us that brain, nerves, and heart, are not necessary to the process of nutrition, which is performed by the vascular system only: they teach us, that *fœtal life* is of a very simple kind, and may be carried on without these organs;—that brainless children are incapable of executing the functions necessary for carrying on existence after birth, for respiration is performed by muscles which require the influence of the brain; but that the functions of the heart, which are dependent on the spinal marrow (as proved by Mr Brodie and Dr Le Gallois), may still go on. But, farther, the case, related by Mr Lawrence, confirms also the fact, stated by Le Gallois, from his experiments on rabbits, that the function of respiration is dependent upon a portion of the *medulla oblongata*: for, in this case, that part of the brain only existed, forming a soft tumour, about equal to the size of the thumb, and the fœtus lived four days, dying probably from want of food, as it was kept from the sight of the mother.

In the *fourteenth* paper, Dr Bateman has given a description, accompanied by a good representation, of a tubercular eruption, pretty uniform in its character and progress, which is one among other cachectic eruptions liable to be treated as venereal. The slight elevation, which gives a tubercular character to what might otherwise be called a *macula*, is well expressed in the plate; but the colour is perhaps too scarlet, and should be somewhat more livid. It is certainly useful to fix, if possible, the diagnosis of those eruptions, which resemble, or have been mistaken for, syphilitic affections, an investigation which, the author believes, will tend to circumscribe the number of eruptions, actually resulting from syphilis.

The next case contains a brief account of a case of bubonocele, which required a second operation, five days after the first, in consequence of a new descent and strangulation through the wound in the groin. It is related by Mr Forster, and terminated successfully. A still shorter paper follows, from Mr Money of the Northampton infirmary, containing the results of the trial of large doses of opium in a case of Diabetes mellitus. This patient, beginning with one grain nightly, gradually increased his dose till he consumed every day twenty-four grains of solid opium, which was taken in five or six portions. This quantity he regularly took for a fortnight, when he left the hospital considerably improved. No lethargic effect was produced by these large doses, and the bowels were not only moderately open, but he had, about every fourth day, rather a smart spontaneous purging; at the same time the urine was gradually reduced from 25 pints to 7 pints in the twenty-four hours, the thirst proportionally diminished, and his strength improved. It was observed, however, that he took no opium at all the night before he quitted the hospital, and his urine was immediately doubled. During the use of the opiate, he was not restricted as to vegetable food; so that this case affords a strong confirmation of the effects of large doses of opium upon the urine in diabetes.

The *seventeenth* paper is a continuation of the same subject, which the author, Mr Brodie, had illustrated in a very valuable essay in the preceding volume of these Transactions, relative to the diseases of the joints. In this communication, he limits himself to some practical observations on the symptoms and treatment of the diseases which affect the *synovial membranes.* Mr Brodie discusses, in four sections, the causes and symptoms of inflammation of the synovial membrane, the treatment of that inflammation, the symptoms which are produced by a morbid change of structure in the membrane, and the treatment of these cases: but the paper does not admit of abridgement. In conjunction with the former, it contributes greatly to the elucidation of the diseases of these important organs.

The late Mr Ware has, in the *eighteenth* essay, left the result of his extensive experience respecting an affection of vision, which, though slight and harmless, he was often called upon, with great alarm and apprehension on the part of patients, to treat; namely, "the *muscæ volitantes* of nervous persons." Three cases are well detailed, as examples of the varieties of this affection, and the symptoms of amaurosis and cataract are stated in comparison; whence the following diagnosis is inferred, "that whenever the appearance of muscæ volitantes is unaccompanied with the sensation of a mist, which more or less obscures the appearance of objects, the conclusion may be safely drawn, that it is not a symptom of cataract; and whenever their appearance is not accompanied with a fixed state of the pupil, it may as safely be inferred that it is not a symptom of gutta serena." The plan of cure is, to relieve the mind from intense application, and from objects of anxiety; to clear the bowels by a brisk purgative; and then to give volatile medicines. Local bleeding, or any other debilitating treatment, is commonly injurious.

Dr Hutchinson, surgeon to the Royal Hospital at Deal, relates the effects of a new method of treating the violent form of erysipelas phlegmonodes, which occurs in the extremities, especially the legs, of seamen. In this formidable malady, he affirms that gangrene and sphacelus, of the integuments, or of the whole limb, not unfrequently deprive the patient of his limb or life, in the course of a very few days, if active measures are not pursued early; and even if gangrene do not occur, the disease invariably terminates in effusion or suppuration between the integuments and muscles. These secretions break down the cellular and vasuclar connections between these parts, and sometimes form immense bags of matter, which are moveable entirely round the limb, even from the ankle to the trochanter; thus depriving the integuments of their nourishment, which necessarily lose their vitality, and leave the muscles without a covering to a great extent. The author contends that these consequences may be prevented, by making free incisions with a scalpel on the inflamed surface, in a longitudinal direction, as early as possible in the disease. These incisions should be made through the integuments and down to the muscles, about an inch and half in length, varying in number from six to eighteen, according to the extent of surface occupied by the disease. Such incisions, the author adds, yield between 15 and 20 ounces of blood, afford great relief to the over-tension of the skin, and form ready channels for the secretions as soon as they occur. This plan, with fomentations, laxatives, and antimonials, generally produces relief within twenty-four hours; and Dr Hutchinson affirms, that he has not lost one case during the last five years, since he adopted this practice; that the recovery was speedy and complete, and that the incisions always healed kindly.

The *twentieth* paper contains a brief description of a case, by Dr Graham, of Glasgow, in which, after death, the aorta was found to be entirely obstructed (but whether recently or from birth was questionable), and the circulation had been carried on by enlarged subclavian, superior intercostals, and mammary arteries, &c. The fact is curious, as evincing the possibility of the continuance of life under an impervious condition of the great arterial trunk, and still farther confirming the doctrine, which has been lately established, that any artery of the body, within reach of the knife, may be tied, without much apprehension respecting the subsequent transmission of the blood. The state of the obstruction is shewn by two drawings.

The *twenty-first* paper is the result of the liberality of the war-office, and of the medical board of the army, to which we have already alluded; it is an " Account of an epidemic fever, which occurred at Gibraltar in the years 1804, 1810, and 1813, taken from official documents, military and medical, and from the communications of Jos. D. A. Gilpin, M. D. deputy inspector of hospitals." In addition to the official returns of the three sickly years, the society has been favoured with the returns of three other years of ordinary health, for the sake of comparison; and also with an account of the weather during the last sickly season, and of the same season in the two preceding years. The principal inferences deducible from these documents are, that the chief medical officers believed the fever to be contagious; that it is limited, like the plague, to a particular season of the year, viz to the months of September, October, and November, at Gibraltar; and that these months, in ordinary years, do not produce more fatality than the other months. The various degrees of mortality in the sickly years, as compared to each other, and to years of ordinary health, are exhibited in tables.

The *twenty-second* paper contains some account of the diuretic properties of the Pyrola umbellata, combined with the history of the disease, which ultimately proved fatal to Sir James Craig, but which was singularly alleviated by this diuretic. The pyrola is found in great abundance in the pine forests of North America: a coloured figure of the plant accompanies this paper. It has been administered in the form of infusion, decoction, and extract, and appears, from a few other authorities quoted by Dr Somerville (who writes this account), to be possessed of unusually active diuretic powers, and to be gratefully tonic at the same time to the stomach.

An interesting communication from the pen of Mr Wardrop follows, in which, after discussing the causes of the prevention of union in fractured bones, and the various methods which have been proposed for accomplishing that re-union; he relates at length the case of a sea-officer, whose thigh-bone was fractured in an engagement, and remained un-united twenty months afterwards. The limb in this state was useless; it was shorter than the other, and he could not move it in the horizontal position, nor guide the foot in the least degree, when he went from place to place, without assistance. Mr Wardrop determined, with the concurrence of Mr Abernethy, to attempt the operation suggested by Dr Physic of New York; namely, to pass a seton between the ends of the fractured bone. The apparatus employed, and the mode of securing the limb, are represented in an engraving. Several untoward circumstances occurred, which are candidly detailed; but at the end of a year, though the cure was not so complete, as was at one period expected, yet the patient considered that the advantages he had gained amply compensated for his sufferings. Mr Wardrop transcribes Dr Physic's case, from the New York Medical Repository; and a communication from Mr Brodie is subjoined, in which the case of a boy, on whom he performed this operation in St George's Hospital, with success, is briefly related. Attacks of erysipelas, beginning near the wounds made by the seton, appear to have been the most formidable opponents to recovery under this operation.

In the preceding volume of these transactions, Mr Travers had furnished an essay on the diagnostics of the various textures of cataract, and the different operations best adapted to each. In the present volume, he has extended his observations in confirmation of the same principles, and concludes the *twenty-fourth* paper, which is entitled, "Further Observations on Cataract," by summing up the contents of both his communications in twelve practical aphorisms, which are too terse for abridgment. These two papers conduce to clear up a considerable degree of confusion which prevailed upon the subject of the operations of extracting and couching, and afford the most rational views of the circumstances under which they are to be respectively preferred.

The real effects of ligatures upon arteries, and the extent to which the anastomosing branches are capable of supplying the place of any large trunk, without even a temporary inconvenience, are discoveries of our own times, and have contributed the means of saving life, in many cases of aneurism, which were formerly left to their natural fatality. The *twenty-fifth* article is a sensible communication from Mr Stevens, of the island of Santa Cruz, relative to the successful tying of the *internal iliac* artery in a negro woman, who had a formidable aneurism of the glutæal artery, after she had been consigned to her fate as incurable. This case confirms the observation already made, that any artery within reach of the knife may be tied with safety. The operation is very distinctly described: one ligature only was used, which came away about the end of the third week, and in six weeks the woman was perfectly well, in which state she continued eighteen months afterwards, when Mr Stevens left the island.

The concluding paper is a brief report from Mr Macgregor, Surgeon to the Royal Military Asylum, at Chelsea, respecting the principal *natural* diseases, as he is pleased to call them, which have occurred among the children, during the first ten years of its existence. By natural diseases, he intends to designate those which are liable to affect most persons once during life. A sort of table is given, containing the actual number of cases of each disease, and the comparative mortality of measles, scarlet-fever, and hooping-cough, after small-pox and after cow-pox. The result is, that none of these three diseases has been more fatal in cow-pox, than in small-pox subjects. The measles have been observed here, as among the children of Christ's Hospital, to be much less fatal when occurring in the summer months, from June to September inclusive, than at other seasons.

This rapid sketch of the contents of this miscellaneous volume will, we doubt not, support the terms of commendation with which we set out. The individual articles are necessarily of various value, partly from the difference of the subjects which they treat, and partly from the variety of talent and opportunity in the writers. But the whole is creditable to the zeal and industry of the Society, collectively and individually. The plates, we may add, are executed in the best style by Stewart.

II.

Observations on the Laws governing the Communication of Contagious Diseases, and the Means of arresting their Progress: (Read before the Literary and Philosophical Society of New-York, on the 9th of June 1814.) By DAVID HOSACK, M. D. F. L. S. &c. &c. 4to. pp. 84. New-York, 1815.

We were for some time at a loss whether to consider the observations before us as an original communication, or as a published book. For the *actual* manuscript we were indebted to Dr Chisholm, to whom they were originally addressed as a letter, and upon the very same day we received them from their author, enriched with numerous notes, printed in a style of luxury which would please the most fastidious lover of *fine paper* copies. By considering it as a book, we are enabled to condense it within moderate limits, and occasionally to add such observations as have occurred to ourselves in perusing it, while the possession of the manuscript entitles us to quote from it without any reserve.

Our readers will recollect, in our fifth volume, a very excellent paper upon the subject of contagion by Dr Hosack, in which he very laudably endeavoured to narrow the ground of controversy concerning contagious and infectious diseases, by arranging them into three classes.

> " The first embracing those diseases which are communicated exclusively by contact; as the itch, syphilis, hydrophobia, &c., which are never conveyed through the medium of the atmosphere.
>
> " The second including those diseases which are communicable both by contact, or the near approach to the sick, and by the atmosphere, as measles, small-pox, scarlet-fever, &c., which are communicable in every season of the year, and in every climate; in a pure as well as in an impure air, though more readily by means of the latter than the former, and with which persons are rarely infected more than once in their lives.
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> " Under the third class are enumerated those diseases which are only, in general, communicable or contagious through the medium of an impure atmosphere; the air being rendered thus impure by the decomposition of animal and vegetable substances, as in low, marshy

countries; or by concentrated human effluvia, as in camps, jails, hospitals, or on shipboard; but the same diseases, I alleged, in a pure air, in large and well-ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges constantly removed, and attention paid to cleanliness in general, are not usually contagious, or, under such circumstances, are very rarely communicated from one person to another. In this class I included the plague, dysentery, typhus fever, in its various forms of jail, ship, hospital, or lake fever, and the yellow fever." pp. 5, 6.

Dr Chisholm, in reply, approved entirely of the two first classes, but requested his friend to reconsider the third; and the present publication is the result of his renewed consideration of the subject.

"In my first communication, I acknowledge I have stated my observations without so full a detail of the facts themselves whence my conclusions were deduced, as perhaps ought to have been exhibited. To the European reader, unacquainted with the peculiarities of yellow fever, more especially as it has appeared in the cities of the United States, my first statement may perhaps appear defective in that evidence which is so justly exacted upon subjects of this nature. This evidence I shall now endeavour to supply, and thereby to confirm the correctness of the classification which has been proposed. Waving for the present all inquiry relative to the nature or properties of the contagious principle secreted by the diseased body, or the chemical qualities of the atmosphere deemed necessary for its propagation, or the manner in which the contagion diffuses itself, I proceed to observe, that the history of each disease enumerated in the third class, viz. plague, dysentery, typhus in all its forms, and yellow fever, furnishes evidence of the correctness of the remark, that they are governed by a law peculiar to themselves; that they are contagious or communicable in a foul atmosphere; but that they are never or very rarely so in a pure air, where the sick enjoy the benefits of cleanliness and ventilation.

"The same evidence, I trust, will demonstrate another truth, that these diseases are in no instances epidemic, as they have been improperly denominated by most practical writers; but that their sphere of operation is, with very few exceptions, confined within the limits to which the vitiated atmosphere extends, in which they may be engendered, or into which they may be introduced; and that, in this respect, they differ from ordinary epidemics, 'which appear in different and distant parts of the same place, and at the same time.'

"That the plague, when once generated, whatever may be the sources whence it derives its origin, is communicated by a peculiar virus secreted by the diseased body, will not, I trust, be questioned at this day. Independently of the facts contained in the writings of Thucydides, Lucretius, Mead, Dr Patrick Russell, and others, showing the contagious nature of the plague, the communication of this disease by inoculation, as performed by Matthias Deggio, Dr Whyte, and the Russian surgeon, noticed by Sonnini, have recently established the fact of its propagation by a specific secretion, beyond all possible controversy." Pp. 7, 8.

Dr Hosack then proceeds to adduce a number of facts to show that the plague, in common with the other diseases associated with it, dysentery, typhus, and yellow fever, is only communicable through the medium of an impure or vitiated atmosphere. We shall quote what he says in regard to yellow fever.

"The facts which have been ascertained relative to the communication of yellow fever, furnish no less conclusive evidence that this disease, like those already noticed, is or is not generally contagious, depending on the qualities of the air to which it may be communicated. The history of every visitation of this disease in the United States establishes this truth. It has not only regularly made its first appearance in our sea-port towns, and in those places where the air is most impure; at that season of the year, and in those seasons when such impurities acquire their greatest virulence; in those houses which are most crowded with inhabitants, and where there is the least attention paid to cleanliness; but, wherever the same disease has been thence conveyed to other parts of the same city or town, or into the country, it either was propagated or extinguished, according to the local circumstances of the place to which it was so conveyed.

"Dr Lining, in his description of the yellow fever, which was introduced into the city of Charleston in 1732, 1739, 1745, and in 1748, observes, that, although the infection was spread with great celerity through the town, yet, if any from the country received it in town, and sickened on their return home, the infection spread no further, not even so much as to one in the same house. He remarks, that the disease was generally more fatal to those who lay in small chambers not conveniently situated for the admission of fresh air. The yellow fever with which the city of New-York was visited in 1791, and which was introduced by a vessel from the West Indies, and rendered memorable by the death of one of our most respected citizens, General Malcolm, who was the first victim to the epidemic of that season, is thus recorded by Dr Jonas Addoms, in his excellent dissertation on that disease:

"'About the middle of August 1791, a contagious fever appeared in the city of New-York, which first discovered itself near Peck-slip, a part of the city thickly inhabited, its houses generally small, and badly ventilated; many of the inhabitants were in indigent circumstances, which is a frequent cause of the want of cleanliness. Here it raged a considerable time; it then began to spread, as some attendants on the sick became infected who lived in other neighbourhoods. By this means it was carried to other families, and most generally could be traced to this source. It likewise proved more particularly fatal near the place where it first appeared than in any other part. Thus at length it spread through the city, until about the middle of October, when the weather growing a little cooler, the disease greatly abated, and in a short time disappeared.'

"Dr Addoms, the author of that dissertation, since that time resided many years in St Croix, and being associated with a celebrated physician of that island, the late Dr Gordon, had ample opportunities of seeing the yellow fever in all its forms. During his last visit to this city, not long before his death, he informed me that the disease which he had seen in New-York, in 1791, was precisely the same which he afterwards saw in St Croix, and which frequently prevailed during his residence there, more especially among Europeans newly arrived within the tropics. He also remarked, at the same time, that this disease always acquired new virulence, and was rendered highly contagious when introduced among soldiers crowded in barracks, or on shipboard.

"In the yellow-fever of 1793, which was introduced into the city of Philadelphia from the West Indies, it is conceded, on all sides, that the disease made its first appearance in Water-street, and that all the cases of this fever were, for two or three weeks, evidently traced to that particular spot. It is also a fact well ascertained, that in the vicinity of the place where the infection was first received, the air was, at the same time, in a very offensive condition, from a quantity of damaged coffee which was exposed upon the dock, and under circumstances favourable to its putrefaction and exhalation. From that place the disease gradually infected a considerable part of the city, the Northern Liberties, and district of Southwark, and did not subside until terminated by frost, after having proved fatal to nearly five thousand persons.

"It is also to be remarked, that its ravages were chiefly confined to the poor, and to those parts of the city where the houses were small, and the least attention given to cleanliness and ventilation. In the language of Mr Carey, 'it was dreadfully destructive among the poor. It is very probable that at least seven-eighths of the number of the dead were of that class. The inhabitants of dirty houses have severely expiated their neglect of cleanliness and decency by the number of them that have fallen sacrifices. Whole families, in such houses, have sunk into one silent undistinguishing grave. The mortality in confined streets, small alleys, and close houses, debarred the free circulation of air, has exceeded, in a great proportion, that in the large streets and well-aired houses. In some of the alleys, a third or fourth of the whole of the inhabitants are no more. The streets in the suburbs, that had the benefit of the country air, have suffered little. It is to be particularly remarked, that, in general, the more remote the streets were from Water-street, the less of the calamity they experienced.'

"'Though the disease,' says Dr William Currie, 'was highly contagious, the influence of the contagion was circumscribed to a narrow sphere.'

"As a further evidence that it did not depend on a general condition of atmosphere, the same author remarks, 'that while this formidable disease was making such ravages in the city, the country, for some miles around, was never more healthy.' In another work, Dr Currie has very explicitly admitted the qualified contagiousness of yellow fever, observing, 'that it is only contagious in situations where the air is confined, and the exhalations of the sick are permitted to accumulate, through neglect of frequently changing the bed and body linen of the patient.'

"Similar facts are recorded of the visitation which New-York experienced of the same disease in 1795. Upon another occasion I shall make public the evidence which is in my possession, indisputably proving the importation of the yellow-fever of that season from Port-au-Prince. In that year the disease appeared upon the east side of the city, first affecting some seamen who had received the infection from a brig directly from Port-au-Prince; from thence it spread in the vicinity from Dover-street to Peck-slip; but throughout that season it was confined, in a great degree, to that part of the town where the local condition of the atmosphere was peculiarly favourable to its diffusion; for not only an unusual quantity of filth was accumulated in Peck-slip, but at that very time a great number of emigrant poor had arrived from England, Ireland, and Scotland, so that the numerous lodging-houses, especially in that neighbourhood, were unusually crowded: add to this, that the weather was uncommonly moist, and thereby peculiarly calculated to spread the infection. According to the statement made by Dr Bayley, it was particularly fatal to the emigrants of that very summer; for, 'out of nearly eight hundred persons who died,' he observes, 'not more than one hundred and fifty were citizens of New-York.'

"In another part of the same statement he remarks, 'so limited was the operation of the contagion, that the number of those taken sick in low situations, compared with those residing in more elevated parts of the city, may be computed as twenty to one.'

"In 1798, New-York was again visited with this scourge of our sea-port towns. During the months of August, September, and October, about two thousand persons fell victims to this disease, at the end of which time, a keen frost put an almost instantaneous termination to its progress. The disease of that season first appeared at the ship-yards in the neighbourhood of New-slip, and, as in former years, was introduced from the West Indies. After cutting off several persons in the neighbourhood in which it commenced, the same vessel was removed to another slip, also on the east side of the city; from thence the disease was communicated by those who worked on board to a thickly-settled part of the city, where the houses are small, the streets narrow, and chiefly occupied by the poor, viz. Cliff-street, John-street, Ann-street, Fair-street, Eden's-alley, and Rider-street; at the same time, however, it still continued to extend its ravages in the vicinity of the place to which the poison had been first communicated, and to some other thickly-settled parts of the town, to which it was subsequently conveyed. In a short time afterwards it was introduced into Pearl-street, and in that part of it between Burling and Peck-slips, where it spread very extensively. In that season a number of circumstances concurred to diffuse the contagion in *that* part of the city; a great quantity of rain had fallen, so as to overflow the cellars in Pearl street, which were, at the same time, stored with salted provisions; these were soon afterwards spoiled, and loaded the atmosphere with a highly offensive vapour; the disease raging at that time in that neighbourhood, acquired new virulence, and, for the most part, followed the course of the vitiated atmosphere; 'beyond the limits of which,' says Mr Webster, 'the disease exhibited little infection.' Indeed, the extension of this disease, as has already been frequently observed, was so circumscribed within the limits of this impure air, that it became very generally believed, that, in that season, whatever may have been the case in former years, the disease exclusively arose from those domestic sources, more especially from the putrid provisions. But that the yellow fever of that season did not derive its origin from the spoiled beef is evident, not only from the fact that the disease had already previously appeared in other parts of the town, and even in that very neighbourhood, before those heavy rains had fallen, and their pernicious effects were perceived; but also, that those tainted provisions, unaccompanied with the specific poison of the disease, did not of themselves communicate infection to those who were constantly exposed to their effluvia.

" Mr Edmund Prior, the inspector-general of beef at that time, informed me, that of forty persons whom he had employed in examining the beef, and in removing and emptying such barrels as were found in a putrid state, not one was taken ill of the yellow fever. But Dr Chisholm and Dr Stewart have abundantly shown, that decomposed animal or vegetable matters will not of themselves produce the pestilence; and that this disease is generated in the human system, and communicated from one person to another, by a peculiar secretion from the morbid body. My object is to show, that when such virus is introduced into a certain state of atmosphere, the disease is readily contracted, but that beyond that atmosphere it is rarely infectious.'

" Although the diseases which have been noticed are rarely communicable in pure air, and are not generally contagious in the country, it is not less true, that in some few instances it appears, either that the virus, as secreted from the diseased body, is alone, in sufficient quantity, or possesses a sufficient degree of virulence, to reproduce such diseases; or that, by means of the impurities collected about the diseased individual, occasioned by inattention to cleanliness and change of clothing, the retention of his excretions, or the confined air of his apartment, the virus itself becomes multiplied, and thereby the means of communicating the disease from one to another are in the same degree increased: for it is a fact not to be questioned, that instances of yellow fever, as well as of the plague, dysentery, and typhus fever, have been occasionally infectious, even in the more pure air of the country, though it must be acknowledged, that such cases are of rare occurrence.

"It is observed by Dr Rush, whose records of the several visitations of the yellow fever in the city of Philadelphia will be lasting monuments of the facts which they contain, as well as of the impressive and eloquent manner in which they are related, "that out of upwards of one thousand persons who have carried this disease into the country from our cities, there are not more than three or four instances to be met with of its having been propagated by contagion." Such instances, however, have occurred in New Hampshire, as related by Dr Spalding; in Connecticut, as stated by Dr William Moore of this city; on Staten Island, in 1798, as recorded by Dr R. C. Moore; at Huntingdon, on Long Island, in 1795 and 1798; and at Germantown, in the vicinity of Philadelphia, as related by Dr Wistar. But these very exceptions, if they can with propriety be denominated exceptions, manifestly prove the specific character of those diseases, and that they are propagated by a specific secretion peculiar to each disease, whether it be plague, dysentery, typhus, or yellow fever. Indeed, to use the emphatic expression of the Edinburgh Reviewers on this subject—'In the present state of medical knowledge, it would not be at all more absurd to deny the existence of fever altogether, than to maintain that it is not propagated by contagion.' But, in the language which Dr Mead has applied to the plague, we may say of all the diseases of this class, 'that a corrupted state of the air is, without doubt, necessary to give these contagious atoms their full force.'

"If it were necessary, I might go on to cite every return of the yellow fever with which the United States have been visited, to show that the progress of the pestilential poison has ever been commensurate with the impurities of the atmosphere, and that, when sufficiently diluted with pure air, it ceases to propagate itself.

"It is probably owing to this impure condition of the atmosphere that the various fevers, and the greater mortality of diseases in general, are to be ascribed, which physicians have frequently observed to precede the appearance of pestilential disorders, and to announce their approach, and which have led many to conclude, that the pestilence itself was thus engendered by local circumstances, and not imported. Facts of this nature have served to mislead the editors of the Medical Repository, and many other late writers, who thus confound the exciting and predisposing causes of disease; who do not discriminate between the inflammable materials and the spark which lights the flame; but have identified the domestic circumstances which have served to diffuse the poison of yellow fever with the peculiar virus itself, by which that disease has been introduced into the various cities of the United States.

"The same local circumstances, I believe, will go far in accounting for the 'pestilential state of the air,' the 'secret constitution of atmosphere,' so often recorded by writers on epidemics; at the same time that they teach us that the diseases now under consideration are only epidemic in as far as the vitiated state of the air is itself epidemic.

"I, however, wish it to be understood, that I do not exclude the influence of bodily predisposition, the passions of the mind, and many other circumstances, in aiding the propagation of pestilential diseases." Pp. 23—32.

From these observations, it appears that Dr Hosack, although of opinion that yellow-fever is really a contagious disease, employs all the facts adduced by the non-contagionists, to prove the domestic origin of the disease, in order to establish that, besides the existence of contagion, an impure atmosphere is necessary for its propagation. In this way he becomes a kind of mediator between these opposite sects, and accounts for many facts, which one or other have never been able to explain satisfactorily; but we verily believe, that none of those who have entered keenly into the controversy will ever become converts. The non-contagionists will never admit the operation of contagion under any circumstances whatever, and the contagionists, though they may admit its becoming more active in a vitiated atmosphere, will find, in Dr Hosack's own publication, abundant proofs, that contagion *alone* is sufficient for its propagation. For example, from his appendix, they will triumphantly quote the statement of facts by Caspar Wistar, Professor of Anatomy in the University of Pennsylvania, in which a vitiated atmosphere seemed not to contribute to the propagation of the disease.

"The disease which produced the fatal effects now to be related, commenced in the family of Elizabeth Johnson, a widow, who lived in the main street of the village of Germantown, about six and a half miles from Philadelphia.

"The person first affected was her child, Betsey Johnson, who had been in Philadelphia from the third to the seventh of August, in a neighbourhood where several cases of the fever had already appeared. She returned home the seventh, and on the ninth of the same month was attacked with the yellow-fever, which terminated fatally in four days.

"Fourteen days after her death, viz. August 27th, Mrs Duy, the next neighbour of Mrs Johnson, who had visited Betsey several times during her illness, was attacked with a fever supposed to be of the same kind, and died at the end of four days.

"On the thirtieth of August, the wife of Charles Hubbs, who also lived near to Mrs Johnson, and had visited both Betsey and Mrs Duy, once at least during their respective indispositions, *but had not been in Philadelphia for many months*, was attacked with unequivocal symptoms of the yellow-fever, in its most malignant form, and died the 2d of September.

"Mr Duy, husband of the above-mentioned Mrs Duy, was attacked sixteen days after the death of his wife, viz. September 18, and died also, after an illness of six days.

"A few days after the death of Mrs Duy, an English gentleman and his wife, of the name of Fisher, who had fled from Philadelphia on account of the fever, went to board with Mr Duy, and were placed in the chamber occupied by his late wife during her illness: they were also attacked with fever. Mrs Fisher was taken, September 19th, and recovered in a few days, but Mr Fisher, who was attacked four days after his wife, died with the black vomit, the 27th September.

"At the same time, the disease reappeared in Mrs Johnson's family, in a young female servant, who was very ill, but recovered. Soon after the attack of this girl, Mrs Johnson herself was taken ill with the same disease: she had visited both of her neighbours, Mrs Duy and Mrs Hubbs, while they were sick, she also had assorted the clothes of her deceased daughter, four or five days before her own attack commenced, but had not been in Philadelphia for a month. Her disorder continued eight days, and terminated the 28th of September, with convulsions and the black vomit.

"A few days before the death of Mrs Johnson, Elizabeth Stern, a woman who lived in the family, was attacked with fever, and became very yellow. Her symptoms appeared moderate at first, but after lingering a fortnight she also died.—The wife of a tenant of Mrs Johnson, who lived in a separate part of the house, but used the same yard, was attacked before the death of Elizabeth Stern, and recovered with great difficulty.

"The last victim to be mentioned, was one Stephen Post, an old man, who lived at a distance, but worked in Mr Duy's barn, while the bed was there on which Mrs Duy died. He was also attacked with fever, and died in a few days.

"These melancholy circumstances occurred in a village which has long been remarkable for its salubrity, at a time when the other inhabitants enjoyed their usual health. In most of the cases, the disease appears to have been contracted at the house of Mrs Johnson, which, before this distressing period, had been eminently distinguished by the health and longevity of its inhabitants. The family were extremely neat, and it may be asserted with confidence, that the premises were never more clean than they were at the time of this truly affecting catastrophe. What cause but contagion is adequate to the production of such a disease among persons so situated?"

The remainder of this elaborate paper is occupied with the exposition of a hypothesis, to account for the manner in which a vitiated atmosphere contributes to the virulence of contagion. In supporting this hypothesis, he mentions some facts, which we are unwilling to pass over.

"The well known facts relative to the communication of *jail fever* to the judges presiding at the Black Assizes, in 1577, and a similar infection being communicated to the judges on the bench, and other persons present, at the sessions held at the Old Bailey in 1750, while the prisoners themselves remained in health, insensible to *infection*, furnish incontestible evidence of the effects of habit in *diminishing* the sensibility to the poison of fever: and with regard to the *yellow fever*, it assuredly has not been the case in the United States, that those who were most accustomed to the impure air of the place in which the disease prevailed, were more susceptible of the dis*order* than those who had recently arrived from the pure air of the country, or from the more elevated parts of the town. On the contrary, those who were least accustomed to the impure air of the city, or of the infected spot, were uniformly observed to be most susceptible of the contagion. Those, too, who enjoyed the most vigorous health, and the most robust constitutions, the reverse of that condition of body which would be the effect of a residence in impure air, were more readily infected upon coming into the atmosphere impregnated with the contagion, than those who had remained constantly exposed to its influence. Whatever differences of opinion have existed among the physicians of the United States, as to the origin of the disease, they are all perfectly agreed relative to the facts which I have just stated. Indeed, Dr Chisholm himself inadvertently admits the same to be true; for he observes, that, in the West Indies, sailors, soldiers, and young men, especially those who have recently arrived from Europe, and are least accustomed to the climate, were more obnoxious to it than others."

"Another argument in favour of this explanation is derived from the fact that this disease has, in several instances, been introduced into our cities, without extending beyond the individuals who have introduced it; manifestly owing to the active exertions of a vigilant police, at the same time that every attention was paid in preserving cleanliness about the persons of the sick. This was remarkably the case in the year 1804, when the yellow-fever was introduced at the Wallabout, on Long Island, and in 1809, when the same disease prevailed at Brooklyn. In each of those years the fever was introduced into this city by persons who had received the infection on Long Island, but, owing to the circumstances just mentioned, it was not communicated to others; while the same disorder, owing to local circumstances, spread in the vicinity of those places on Long Island where it had first appeared.

"During the year 1811, the yellow fever was also introduced into the city of Amboy, New Jersey, from the Havanna, but did not spread beyond those persons who were first attacked in consequence of their immediate exposure to the air of the infected vessel. The local circumstances of Amboy, its elevated situation, its dry and sandy soil, its wide streets and spacious houses, their distance from each other, and the remarkable cleanliness of the town, most satisfactorily account for the sudden extinction of the disease, while the evidence of its importation must be admitted to be conclusive.

" But there is another circumstance which particularly merits attention: in every epidemic visitation of the yellow-fever, several days, viz. from eight to twelve, or fourteen, have generally elapsed between the first cases that appeared, and the communication of it to other persons, even in the same neighbourhood; insomuch that not only our citizens, but the physicians themselves, have been led to doubt the existence of the disease, and to stigmatize as alarmists those who first announced the deadly visitor. I can never forget the occasion, in 1795, when that venerable and experienced physician, the late Dr John Bard, assembled the physicians of this city to announce to them the first cases of this disease which he had observed in the family of his friend, Mr Jenkins. The physicians met, but declared they had seen no other fevers than what they had been accustomed to observe every year, and even doubted, on that occasion, the correctness of Dr Bard's observations, relative to the nature and character of the disease to which he called their attention: but that accurate observer had been too familiarly conversant with the yellow fever as it appeared in New York in 1743 and 1762, and too well knew the pathognomonic symptoms of that disease to confound it with the fevers of our own climate: he, accordingly, in the most emphatic language, replied to their doubts, " Gentlemen, within a fortnight you will all see and acknowledge the West India yellow-fever to exist in our city." The event is well known. The same interval between the first cases of the disease and its subsequent diffusion in the neighbourhood where it first made its appearance, is noticed by almost every writer who has recorded the yellow-fever in the United States.

" A similar interval has been frequently noticed in the history of the plague. Dr Russell, in his account of the plague of Marseilles, in 1720, observes, " that from the 12th of July to the 23d there was a deceitful pause, during which the popular apprehensions began to subside. The physicians were reproached with ignorance in having mistaken ordinary fevers for the plague. The disease, however, in this interval, had continued to spread in the street, Rue de l'escale, where it made its first appearance."

" It has also been remarked of the plague, as well as of the yellow-fever, that the infection spread most rapidly when the atmosphere was not only heated and loaded with moisture, but when it was least agitated by wind or thunder storms. During those calms when the air may be said to be relatively at rest, it has been uniformly remarked, that the contagion of the yellow-fever has multiplied itself most extensively, as was always very apparent by the greater number that was seized within five or six days after such close weather had been observed, all which circumstances certainly conspire to promote the fermentative process that has been contended for.

" This is not all: whenever the yellow-fever has been introduced into the cities of the United States, its first extension has always been slow and gradual. Upon several occasions its boundaries have been accurately defined by our board of health. This, as I have stated on a former occasion, was remarkably the case in this city in 1805. The disease, in that year, was confined, for some weeks, to a small portion of the eastern side of the city, and, as stated by the board of health, " not a case occurred, in any part of the town, that was not referable to that as its source." This fact being ascertained, the board accordingly forbade intercourse with the infected portion of our city, and ordered an abandonment of that part of the town, threatening violent measures if their orders were not immediately complied with. In a short time after, the infection extended a few streets farther; the board of health again defined its limits, and again declared that still not a case had occurred that could not be traced to this part of the city as its source.

" Will not the same assimilating or fermentative process furnish the most satisfactory solution of the fact noticed by Boerhaave, Cullen, Lind, Russell, and many others, that *fomites* are more to be dreaded than the excretions alone proceeding from the diseased body? Not, however, in the manner those authors suppose, that said fomites acquire greater virulence; but that by the same process the specific poison has been more extensively multiplied by means of the atmosphere and foul excretions which are involved in the clothing worn by the sick; and that by the same means the danger of the infection has been increased in the same degree that the poison has been multiplied. As a further evidence, too, that the contagion is *multiplied*, but not more concentrated, as those writers have imagined, it is a fact established by every writer on those contagious diseases, that the first cases of every epidemic are uniformly the most fatal; but that, as the season advances, the danger of taking the disease is increased, while the disease itself has, perhaps, become even milder than it was in the commencement."

From all these facts and observations, Dr Hosack draws the following conclusions:

" 1st, That an impure atmosphere is indispensably necessary to multiply and extend the specific poison constituting *plague*, *dysentery*, *typhus*, and *yellow-fever*.

" 2dly, That the impurities of the atmosphere do not produce their effects, in the manner suggested by Dr Chisholm, by increasing the susceptibility of the system to be acted upon by the peculiar virus of those diseases.

" 3dly, That, instead of predisposing the body to be thus acted upon, the reverse is the fact; that the predisposition of those who are most exposed to such impure air is less, while those who reside in the pure air of the country are most, liable to be infected when exposed to the contagion.

" 4thly, That the impurities of the atmosphere are fermentable materials, to be called into action by the specific ferment of those diseases, aided by heat, moisture, and a calm state of the atmosphere, and that, as far as such atmosphere extends, and the circumstances favourable to such fermentative or assimilating process continue, so far as these diseases become epidemic, but no further."

For his inquiries into the laws of contagion, so far as they have gone, we acknowledge ourselves indebted to Dr Hosack; and we are persuaded that many an acrimonious controversy would be avoided if they were better understood; but there still remains a great deal to be discovered and ascertained in regard to them, for, unluckily, most of those who have written concerning them come prejudiced to the investigation; and, instead of deriving general laws from an extensive view of facts bearing upon the subject, they rather endeavour to render plausible some position which may support their opinion concerning the contagiousness or non-contagiousness of a particular disease. This is one of those subjects where an *a priori* consideration may be advantageously connected with a reference to facts. We mean, that by *pure reason*, as a Kantian would term it, a scheme of every possible case, whether in regard to classification, or to circumstance, or law, might be drawn out, and afterwards filled up with such facts as were applicable to each. In this way we would discover in how many important points our information was still deficient, and how much of our scheme we should be under the necessity of leaving blank.

III.

Delineations of the Cutaneous Diseases comprised in the classification of the late Dr Willan: being a Republication of the greater part of the Engravings of that Author, in an improved state, together with a New Series, which will comprehend the remainder of the System, as completed in the Practical Synopsis of Dr Bateman: the whole being intended to illustrate the Principal Genera and Species described in that Work. By T. Bateman, M. D. F. L. S. Physician to the Public Dispensary, and to the Fever Institution. London, 1815.

This work does not stand in need of our commendation. The utility of the study of cutaneous diseases cannot be disputed; and as conducive to that important end, the publication of Dr Willan was unrivalled, not only in this, but in any other country. German industry has not yet been directed towards this subject; and the costly work of Alibert only proves how unfit he is for the task he has undertaken. In regard to Dr Willan's work, we had first to regret the slowness of its progress, and finally, to lament his death when he had only completed one half of his arrangement. We had indeed little doubt that some of his pupils would endeavour to complete the system so satisfactorily begun; but we felt anxious lest it should be attempted by one who did not come sufficiently prepared for such an undertaking. The publication of Dr Bateman's synopsis, terminated our fears on this subject; for although the nature of his work did not admit of that display of learning and illustration so honourable to his illustrious predecessor, it left us nothing to desire, in regard to the practical knowledge of skin diseases, so far as it could be conveyed without the assistance of graphic delineations.

The *Synopsis* is now in every physician's library who pretends to any diagnostic skill; but accurate as the descriptions are, language is so inadequate to conveying a perfect idea of objects of sight, that every one, who has had frequent occasions of consulting the work, has had occasion to regret the want of plates. This is particularly the case, in regard to the four last orders, which Dr Willan had left untouched. It is therefore with extreme satisfaction that we have to announce, that Dr Bateman, who has so well completed the text of Dr Willan, has begun to do the same justice to his delineations. Instead of commencing where Dr Willan left off, he has begun at the beginning of the system, and we think he has judged wisely; because what Dr Willan had published, was no longer to be purchased in the shops; and because it renders the whole system uniform, and enables Dr Bateman to introduce corrections and improvements wherever they seem necessary. The nature of the present work, its extent and mode of publication, are so distinctly stated in the advertisement prefixed to the first number, that we transcribe it.

"From the many urgent representations that were made to me, while I was preparing my Synopsis of Cutaneous Diseases for the press, respecting the deficiencies of a mere descriptive treatise, unaccompanied by any graphical illustrations,—and, from the obvious advantages which an ocular view of these external maladies affords, in conjunction with verbal description,—I am induced to suppose that a publication, like the present, will be acceptable to most of the readers of the Synopsis. With a view to their convenience, these plates will be published separately from the text of that work, of which they are intended as an illustration; a few words of description and diagnosis only being annexed, for the purpose of rendering the plates not altogether unintelligible alone, but which will add nothing to the *cost* of the work.

"Upon this subject, I may observe, that it would have been more satisfactory to myself, if the Fasciculi could have been published at a lower price; but it would have ill answered the purpose, either of the editor or the purchasers, to sacrifice accuracy to cheapness; or to incur the risk of misleading, and being misled, by imperfect and ill-executed representations of the diseases attempted to be pourtrayed, merely to facilitate the sale of the work. I trust, however, that the appearance of a Fasciculus four times in the year, for three successive years, will not be deemed too frequent, nor prevent my professional brethren from taking a share in the necessary expences of such a work.

"The copperplates, belonging to Dr Willan's Treatise, having come into my possession, together with several drawings, prepared for the subsequent orders upon which he had not published any thing; and many original drawings, relative to the whole classification, having also been procured; I propose to publish twelve Fasciculi, containing six plates each, illustrative of all the principal genera and species, described in the Synopsis. The first four orders, consisting of Pimples, Scales, Rashes, and Blebs or Blains, (Bullæ) will be illustrated chiefly by the republication of Dr Willan's plates, improved at a considerable expence by the able hands of Mr Stewart, and tinted with every possible care. The syphilitic eruptions, being a distinct class of diseases, and being, in fact, very insufficiently ascertained or characterized, will be, for the present, omitted; and a few additions, illustrative of other varieties, more immediately belonging to the arrangement, will be made. The last four orders, comprising the Pustular, Vesicular, Tubercular, and Macular diseases, will be illustrated by a series of new engravings, executed from the original drawings above-mentioned. This collection of drawings is nearly completed, and a few of them already engraved; so that punctuality in the completion of the publication may be confidently promised."

We have only a few words to add regarding the mode in which these two fasciculi are executed. We have compared them carefully with Dr Willan's original plates, and we have no hesitation in pronouncing them to be considerably improved, chiefly in the truth of the tinting, which on this subject is of immense importance. None of the plates has been left altogether untouched by Mr Stewart's graver; several are a good deal altered, and one reingraved. A work of this nature, we are well aware, affords no immediate pecuniary remuneration to its author; but Dr Bateman, like his predecessor, will gain by it a much more honourable, and ultimately more valuable reward.

IV.

Part Second. An Essay on the Venereal Diseases, which have been confounded with Syphilis, and the Symptoms which exclusively arise from that Poison. Illustrated by Drawings of the Cutaneous Eruptions of true Syphilis, and the resembling Diseases. By RICHARD CARMICHAEL, M. R. I. A. President of the Royal College of Surgeons in Ireland, and one of the Surgeons of the Lock Hospital, Dublin. 4to. pp. 116. Longman and Co. London, 1814.

AT length we receive the concluding portion of Mr Carmichael's work, which we noticed at some length in the number for January, together with the plates illustrative of the whole treatise. But the more we see, and the more we examine the subject, the more we are convinced that our author has made a premature grasp at generalization, and that his doctrines will not stand the test of experience, but will prove to be at variance with more extended observation. Satisfied as we are, that numerous local and constitutional affections are perpetually pronounced to be syphilitic, and treated with mercury, which are not venereal, and are even aggravated by that medicine; yet we cannot look upon the first plate of Mr Carmichael, on which two eruptions only are delineated, and assent to the statement, that no other eruptions than these require the full impregnation of mercury for their cure, or are the result of the true primary symptoms of syphilis; nor do the other three or four cutaneous appearances, which he has delineated, comprize a twentieth part of the various affections of the skin which are seen in the cachectic states, which follow primary ulcers in some part of the body, or arise where no such ulcers have been seen. We believe, too, that, when he ascribes that opinion to "*mistake*," which maintains that the excessive use, or the peculiar operation, of mercury does produce "eruptions, ulcers on the skin and throat, and nodes on the bones," (p. 220.) which are cured by the omission of the mercury, and the substitution of other means, he applies the word *mistake* to the sober inferences of some of the most experienced practitioners of this country, whose opportunities, both in public and private practice, for a long series of years, have been probably superior to his own.

Indeed we conceive that his experience must have been very limited, whose personal observation has not satisfied him of this fact. But to the work.

Our readers will recollect that, in the three chapters, which constituted the first part of this work, Mr Carmichael discussed briefly the nature of certain morbid poisons which have some relation to syphilis, the yaws, sivvens, &c. and marked the existence of *venereal* diseases (i. e. diseases communicated by venereal intercourse) which had occurred previous to the appearance, and are distinct from the character of *syphilis*, or that malady which mercury alone cures. He then endeavoured to prove that the *syphilitic* poison produced the primary symptom, chancre, and the secondary eruption, of a *scaly* character, and no other: and in the 3d chapter, he described the *gonorrhœal* poison, which he considered as manifesting itself *primarily* in three ways, viz. as a superficial ulcer, without elevated edges or induration,—as an excoriation of the glans and prepuce,—or by the proper gonorrhœa from the urethra: and under each of these forms, he contended, it was followed by one and the same eruption, a Lichen, or *papular* eruption. He also mentioned another superficial ulcer, with elevated edges, but which, not being followed by any secondary symptoms, ought not to have been classed with the gonorrhœal, and need not claim any farther notice here. But besides these two diseases, the *syphilitic*, with its chancre and *scaly* eruption, and the *gonorrhœal*, with its excoriation, discharge, or superficial ulcer, and *papular* eruption; he promised to describe, in his fourth chapter, another poison, which produces the *phagedænic* ulcer, and perhaps the *sloughing* ulcer, and also its peculiar secondary eruption; and in his fifth chapter, those secondary or constitutional symptoms, which have not been traced to any primary ulcers. These two chapters, then, constitute the Second Part, which is now before us.

The fourth chapter treats of the "second class of primary diseases, which have been confounded with syphilis, consisting of two species,—the phagedænic ulcer,—and the sloughing ulcer.—Constitutional symptoms arising from these ulcers." It seems manifest from this chapter, that the experience of our author, which he deems sufficient to develope the nature of these obscure maladies, which have employed the life-time of many an able observer, is very brief and of recent date. We find most of his cases of no earlier date than 1812, two years only before the commission of this treatise to the press: and so late as the summer of that year, we find him first discovering that these phagedænic ulcers, and certain other corresponding affections of the throat and limbs, were not relieved by mercury, but were aggravated during its use; and then first adopting "a new view of the subject," which, however, is perfectly familiar to experienced surgeons, namely, the necessity of abandoning the administration of mercury, and of resorting to sarsaparilla, good air, and other means of improving the general health, under such circumstances. Mr Carmichael seems to consider these phagedænic ulcers, whether primary or secondary, as the result of a peculiar poison, which is not syphilitic; and the constitutional symptoms, with the affections of the throat, palate, nose, &c., as the regular consequences of the ulcer on the genitals; to which he also adds a particular form of cutaneous eruption. But we do not think the evidence of his cases bear him out altogether in this opinion. Almost all the cases were of some standing before the author saw them, and *had been treated with mercury*, often to a considerable extent. The probability is that many of them were truly syphilitic cases in the outset; but that the syphilitic virus had been eradicated by the specific, and these phagedænic dispositions had ensued, either as "the sequelæ of Lues venerea, or the effects of mercury in a bad habit of body," to borrow the words of an author very well informed upon this subject. * For we are disposed to believe, with this able and experienced writer, that "the venereal virus, when introduced into the system, often gives rise to morbid appearances, which do not in any proper sense partake of the nature of the remote cause; and even symptoms originally venereal may lose that character, by the proper use of mercury, while, to the eye of a common observer, they seem not to have undergone any favourable alteration." And farther that, by the improper use of mercury, such cases are made to assume more formidable symptoms; and especially that those wasting ulcers, both of the genitals and fauces, are thus often occasioned, we believe the same experience will bear us out in maintaining. And it is in this way, doubtless, and not on the supposition of perpetual mistakes as to the original nature of these diseases, that we are to explain many of the well attested cures, effected by guaiacum, sarsaparilla, cinchona, &c. after mercury had failed to arrest the symptoms; as well as the actual success, with which certain empirics have treated many cases, by their vegetable remedies, without mercury, after this remedy had been tried too long by the regular practitioner.

With respect to the eruptive disease, which the author asserts to be peculiar to this phagedænic ulcer, in common with the sloughing ulcer, which he next describes, it appears to us that nothing approaching to proof of its identity, or of its exclusive connection with these ulcerations, is here stated. He has figured a pustular and scabby eruption, the form of which is by no means definite; and in two or three cases, in which an eruption is said to have followed the phagedænic ulcer, any sort of crust, scab, or scabby ulcer, appears to be deemed a specimen of the *same eruption*. "A circular crust the size of a shilling, situated on his forehead, with a surrounding red pimply eruption," p. 175; "pustules scattered over her body, but more numerous on her back, than on any other part, which were of a large size, and as distinct as those which occur in a case of mild small-pox," p. 177; "three spots on his face formed by large brown conical crusts, one of which, in particular, projected like a horn from his forehead," p. 179; "a large spot covered with a crust on his right elbow, which he states to have been there a fortnight," p. 147:—these are all given as specimens of the same identical eruption, the secondary product of the two ulcers just mentioned! In one case, indeed, p. 179, an eruption is figured as a specimen of this secondary symptom, though it is only *conjectured* to have been preceded by one of these ulcers, from its alleged similarity to another case, no primary symptom having been in reality seen! In almost all the other cases, related by the author, a sore-throat, without any cutaneous eruption, was the principal secondary symptom of these ulcers.

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* Mr John Pearson, in his admirable work on the Effects of various articles of the Mat. Med. in Lues Venerea. Introduction.

It is very obvious that this is not a specimen of induction of a very philosophical character, and that the inferences cannot be considered as legitimately established. We have no doubt, indeed, that, after farther experience, the author will not continue to maintain this doctrine; and that eruptions of this kind often occur, with ulcerated throat, and pains in the limbs, where no phagedænic ulcers have preceded, and are much more varied in their character, under both circumstances, than he is disposed to believe. Neither have we any doubt of the occurrence of pustular and scabby eruptions, as the secondary symptoms of true syphilis, curable by mercury alone; we are rather inclined, indeed, to consider the tendency to ulceration, if not arrested by the specific, as inherent in all the syphilitic eruptions, both upon the authority of sound and long-experienced observers, and of our own remarks.

We do not mean to contend, however, that these phagedænic and sloughing ulcers of the genitals do not occasionally occur, independently of the poison of syphilis, or of mercury, or of the combined operation of both; or that they are not sometimes communicated by venereal intercourse, where no actual syphilis exists; but merely that Mr Carmichael has attempted to generalize much too hastily from a limited number of facts. On the contrary, we have little doubt that he is right in maintaining that the formidable disease, which was communicated to the British army in Portugal, as described by Mr Ferguson, in the Medico-Chirurgical Transactions (Vol. IV. Art. 1.), and which, while it is cured by the natives without mercury, produced many dreadful mutilations among the British, while that remedy was resorted to, was of this nature, and not the true syphilis. Although, therefore, we cannot follow Mr Carmichael as far as he has gone, we cannot but deem his statements as of considerable value to the profession, by calling the attention of practitioners to points of distinction, which they are apt to overlook, and of the existence of which multitudes in the profession are, we believe, ignorant: and they evince, on his part, great zeal and activity of observation, and probably some originality; but, as far as any truth is established, less of real novelty, in the eyes of men of experience in this country, less in the estimation of those who have been educated in the school of John Hunter, or in the later schools of Pearson and Abernethy, than he appears, from the hastiness of his publication, to suppose.

The *fifth* chapter treats of those "constitutional symptoms which have not been traced to their primary ulcers," and includes an "inquiry into the probability of the spontaneous origin of some diseases which bear a resemblance to the constitutional symptoms of syphilis." We confess that we are a little surprised to find the author setting out with this annunciation; that, "the task will not be very burthensome, for *one species of eruption only* remains to be discussed; all the others which I have met with having been already described, and traced to their source." (p. 187.) We began to recollect the extreme variety of cutaneous eruptions, which we imagined our own observation had presented to us, of equivocal character, and accompanied by nodes, and pains of the limbs, by ulcerations of the throat, inflammations of the eyes, emaciation, &c.; and we remembered the difficulties expressed by one of the most learned and able surgeons of the age, already quoted, after an attendance of 30 years on the Lock Hospital of London, and the enjoyment of extensive general practice, respecting these spurious appearances: "I have not yet attained to that complete and satisfactory knowledge of the *cachexia syphiloidea*, which would authorize me to obtrude a publication on the subject; but the experience I have already had in the treatment of that *multiform disease,*" &c. &c.* The inference must be, that Mr Carmichael has not seen, or not marked, these *multiform* appearances, and is therefore generalizing from a very limited collection of facts; or that the philosophical observer, just quoted, has been in the path of self-deception during his career of investigation, and that we are quite mistaken in our notions of discrimination as to these eruptions. We think, indeed, that the mere inspection of *four engravings*, containing at the most *six* forms of eruption, which are said to be all that the author has ever seen of secondary symptoms, syphilitic, or venereal, or the resembling eruptions, would be sufficient to settle the question, with any attendant on a venereal hospital, or ward, of a few years standing. We have little doubt, that eruptions, not distinguishable from each other in external character, are sometimes syphilitic, and sometimes not; and the first syphilitic eruption, figured by Mr Carmichael, is probably in this predicament. It appears to be the same eruption, which has been recently figured in the Medico Chirurgical Transactions of London (Vol. V. Art. 14.) as one of those equivocal diseases, which is often curable without the assistance of mercury.

After a considerable detail of cases, of which, indeed, this second part principally consists, we expected to find the "inquiry into the probability of the spontaneous origin of some diseases which bear a resemblance to the constitutional symptoms of syphilis," (to which, the author observes, his cases naturally lead, and which he announces in capitals),—we expected to find this inquiry, we say, occupying the author's most particular attention. We were, therefore, not a little disappointed to find, that he absolutely avoids the inquiry altogether, contenting himself with referring to Mr Abernethy, and stating briefly the general fact, that the health is in all cases previously disordered, and the chylopoetic organs especially deranged. But, with all deference to the acknowledged genius of Mr Abernethy, which is entitled to the highest respect, it does not appear to us that much light is thrown upon the pathology of diseases, by an observation so vague and general as this. Is it one and the same disorder of these organs, which produces all the variety of local affections? or are there varieties in this chylopoetic derangement productive of the varieties of local disease, which therefore require discrimination, and a difference of remedy? So far as this observation implies the necessity of employing constitutional remedies for the cure of local diseases, it is of the utmost practical value; but inasmuch as it tends to an empirical prescription of a given remedy, for the purpose of rectifying some assumed uniform derangement of the digestive organs, be it of the stomach, or of the bile, under every circumstance of local disease, which is not the result of obvious causes, insomuch it tends to fetter pathological inquiry, to supersede the labour of observation, and to reduce the practice of medicine to the administration of a nostrum. Mr Carmichael contends, that this doctrine is supported by his own observations respecting scrofula, which he believes to be the immediate result, in a great majority of cases, of disorder of the chylopoetic viscera. But scrofula, and these syphiloid maladies, are not the same diseases, and are not curable by the same remedies. Where then is the philosophy, or the practical benefit, of imputing to them the same origin, in these vague hypothetical terms? The empirics of old were right, when they observed, "Sed has latentium rerum conjecturas ad rem non pertinere, quia non intersit, quid morbum faciat, sed quid tollat." (Celsus, *præf.*) Hypotheses which are merely expressions of some very general fact, and lead to no discrimination in practice, are, to say the least, irrelevant: ad rem non pertinere.

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* Pearson, loc. cit. p. liv.

We do not think, however, that our readers are yet acquainted with the extent to which our author's hypotheses sometimes carry him. "More than one morbid poison," he says, "is superseded or suspended by the inoculation of the cow-pock;" a circumstance which yet remains, we believe, to be proved. But taking it for granted, he thus philosophically infers, (and immediately sets about the experiment), that one of these vile poisons *may probably supersede another*, and that the virus of syphilis may be *inoculated as a remedy* for these resembling diseases, of more obstinate malignity. In one sentence, the author calls this "*a rational conjecture*," which "struck him most forcibly;" and, although in the next he characterizes it, very justly, as "a strange proposal," he carries it into instant execution. He *vaccinated* two patients, who had been long labouring under the syphiloid cachexia, not preceded by any known primary symptoms, but principally affected with ulcerations of the throat. They had had the small-pox, and the vaccination produced no material effect. He then inoculated both patients in the thigh, with matter taken from small ulcers on a man's prepuce, which, though they then looked like incipient chancres, turned out to be of the second class; *i. e.* of the nature of the gonorrhœal poison. This took place, October 21, 1814. In one patient, the inoculated part on the 7th of November had thrown off its crust, and presented a sore as large as half-a-crown. The ulcer of the throat had continued to extend to near this time, and formed "the most extensive and malignant ulcer" that the author had ever seen in the throat. The patient (*patient* indeed!) had pains in his shoulders and arms, "characteristic pains," says the very accurate author, "of the smooth superficial ulcer." During the remainder of November, this abominable ulcer seems to have "improved," and the inoculated spot was healing. But, on the 13th of December, the last day before this manuscript was sent to the press, "the ulcer had extended itself again over the newly cicatrized parts," and there the author leaves it. In the other patient, who had for nearly two years had ulcerations on the genitals, and been several times salivated, and recently under mercurial action, had a tubercular scabby eruption and ulcerated throat. We have brief reports of the case after the inoculation, from October 21, to December 16, when the author was obliged to print, at which time the account was a little more favourable than in the last case, but the disease was never so severe; the ulcer of the pharynx was not healed, but said only to be looking healthy.

Mr Carmichael, acknowledging the imperfection of these unfinished cases, thinks, nevertheless, that it would have been unjustifiable to withhold "from others *so promising a field of investigation!*" If we were not sure, however, that no practitioner on this side of the water would be misled by such unfounded, irrational, and dangerous suggestions, the mere whims of the brain, unsupported by analogy in the first instance, or by any thing approaching to evidence in the second, we should raise our feeble voice to deprecate such unjustifiable tampering with morbid poisons, which often baffle all our powers of remedy, when accidentally introduced, and constitute the sources of some of the most formidable maladies of mankind. We deprecate all *wanton* experiments upon human health and life; and we doubt not that every sober-minded reader will deem that epithet most appropriate to such suggestions as have just been stated. And as to the proposal which the author next makes, of inoculating the last mentioned patient with syphilitic virus, and putting him on a mercurial course, as soon as secondary symptoms shall appear, it seems to us still more hazardous, and unsupported either by analogy or by reasoning. The most important step towards the cure of such patients as this, who have been under many successive courses of mercury, is to get rid of all mercury from their constitution; for although it produces temporary benefit, it gradually destroys the vigour and health, and prevents the ultimate cure. We have seen mercury prove an actual cordial in some cases of this kind, its temporary relief was so great; but, to save the patient, it was necessary to interdict the mercury, and use every possible means of restoring the constitution. Yet, in such cases, are we to adopt, upon mere vague speculation, the infliction of a poison, which requires this bane, mercury, for its own antidote? Is it probable that mercury, which has hitherto worn down the health, will be harmless, because it has another malady to act upon? And what rational ground is there for supposing, that syphilis is capable of superseding the diseases which resemble it? Is syphilis, when inoculated into the arm or thigh, less virulent than when inserted into the glans, or the prepuce? Or is there the shadow of any other analogy between such an inoculation, and that of small-pock or cow-pock.

On the whole, we are disappointed with the conclusion of this work, though we, in some degree, anticipated the impression which it would produce. We are fully persuaded, that the experience of a few years of any man's life, or of any hospital, is totally inadequate to the collection of even a small portion of the anomalous and "multiform" phenomena of syphilis, and the diseases which have some affinity to it; and that any attempt to deduce general laws from such an imperfect accumulation of facts, must be futile and short-lived. We believe, too, that in the eyes of the distinguished practitioners of experience in this country, very little that is truly established in this treatise will be new, and that they could refute some of those novelties, by a host of facts of which we are not possessed. Nevertheless, we shall recommend this treatise to our readers, as a repository of valuable cases, and as contributing to accelerate the progress of opinions relative to the treatment of diseases, which are constantly mistaken for syphilitic affections by the majority of those practitioners, whose education has been somewhat limited; and as tending to open the eyes of many to the actual variety, both of primary and constitutional symptoms, in which mercury is not required.

V.

Facts and Observations on Liver Complaints, and Bilious Disorders in General, &c. &c. By John Faithhorn, formerly Surgeon in the Honourable East India Company's Service. London, 1814. 8vo, pp. 148.

BOOKS, medical as well as others, are written from various motives. Mr Faithhorn evidently writes to get patients.

But we do not condemn him on this account. It is a very general motive with medical authors, although almost always attempted to be concealed; and it is honourable, or the reverse, according to circumstances. It is dishonourable to pretend to superior skill, when we do not feel conscious that we possess it; and even when we do, it is not considered as consistent with the liberal exercise of the profession, to advertise that skill, unless we at the same time make an attempt at, or a pretence of, communicating it to the profession at large. On the other hand, there is no way of getting into practice, so honourable to ourselves, or so useful to others, as the publication of a work which evinces knowledge of the healing art in general, or of any branch of it in particular.

If we assume, as the criterion by which to judge of our author's motives, his writing for the profession, or to the public, we much fear that Mr Faithhorn will not add to his reputation by the publication before us. Indeed we discover an inconsistency in the author's statement of his reasons for writing, which is at best extremely suspicious. In the preface to the book, he tells us,

> "The following work is presented to the Profession, and to the Public, not for the purpose of advancing what is already known on the nature of Liver Complaints, and Bilious Disorders, or to detract from the merit of the authors who have already preceded on this subject, but to point out to their attention *such minutiæ*, in regard to the symptoms and *treatment* of these maladies, which either have been overlooked, or met with but an incomplete or careless investigation :"

While in the preface to the notes he tells us quite a different tale.

> "In the *treatment*, he has only entered into general principles. This is sufficient for the profession, and for the public it is unnecessary to enter into *medical minutiæ*, which they could not of themselves carry into effect."

What follows and precedes this latter quotation explains the real object of the publication. It is to alarm his readers in regard to the frequency and importance of bilious diseases, while at the same time it informs them of his great practical skill in removing them.

> "It is, by experience, in a tropical climate, the real nature of hepatic affections, and bilious disorders in general can be best comprehended, and their successful management more correctly understood. It is here diseases advance with an almost incredible force, their effects

are most serious; and, an active and energetic use of medicine is the only resource, to check their rapid and alarming progress: It is in this school, then, a practitioner has the most enlarged opportunities of making his observations. He learns here to apply his knowledge with boldness and effect, and instead of trusting to the weak and inefficient efforts of nature, as in colder climates, he is forced to a different line of conduct, to meet them promptly with all the instruments which experience and observation render him master of. It was in a warm climate these truths were first impressed on my mind, and they have acquired additional strength since my return to my native country."

But although it is only in a tropical climate that the knowledge of bilious diseases can be acquired, luckily for Mr Faithhorn, he has discovered that they are no less common here.

"Ever since my return, this subject has particularly engaged my attention and my own observation, as well as those of my medical friends who have been in the East Indies, support me in the assertion, that liver complaints and bilious disorders, are as frequent here as in the warmer latitudes; but here they assume a different character; their appearances are less distinctly marked; the vitiated secretion does not shew the same rapid progress and strong apparent change, though the derangement it produces is not less severe or active in the destruction of health. It is to trace then these latent, these insidious and less obvious forms of attack so often mistaken, and that too fatally for patients, that the present work is intended."

The professional reader will in vain search this treatise for any information, unless he be an admirer of such farragos as the following:

℞ Pulv. Rhei.
Potass. Subcarb. āā ʒss.
Magn. Carb. ℈i.
Tin. Sennæ. ʒij.
Sacch. Alb. ℈iv.
Sp. Ammon. C. m. xxxx.
Aq. Distil. ℥iv. m. ft.
Mist. Sumr. dimid. cras primo mane et reliq. 2da hor. postea.

VI.

Observations upon the Bulam Fever, which has of late years prevailed in the West Indies, on the coast of America, at Gibraltar, Cadiz, and other parts of Spain: with a collection of Facts proving it to be a highly contagious disease. By WILLIAM PYM, Esq. Deputy Inspector of Hospitals, late Superintendent of Quarantine, and President of the Board of Health at Malta. London, 1815. 8vo, pp. 299.

When we find such diversity of opinion, and even such opposite statements of matters of mere observation among those who appeal to personal experience in favour of their assertions, we need not be ashamed to confess that we, who can only judge of *yellow fever* from the reports of others, are often perplexed, and inclined to vaccillate in our decision. One advantage we however possess over these experienced witnesses; we have imbibed no prejudices,—we have espoused no party,—our feelings are not engaged,—and our opinion remains either doubtful, or is formed upon a cool consideration of the facts and arguments adduced by the advocates of every party. It requires a great deal of candour in a person who has formed his opinion, as he believes, from what he himself has seen, to renounce that opinion in consequence of the increased or corrected observation of others. It is like consenting to hear and see with the ears and eyes of another, when our own senses give us contrary information on the subject. And yet when we reconsider our experience, how often have we been obliged to admit the inaccuracy of our observations,—or rather how often have mistaken for observation what was in reality inference, and erroneous inference. It is in this manner we are convinced that many supposed contradictions in matters of observation are to be explained; and we are willing and justly entitled to give full credit to the veracity both of the knight who asserted the shield of the statue of victory to be gold, and of him who asserted it to be silver. Our veracity is not called in question; our passions are not roused; and we will examine both sides coolly and deliberately.

We have repeatedly expressed our regret that we had only *newspaper* notices of the dreadful malady which committed such devastation in Gibraltar, and we even censured the apparent reluctance of the medical officers of that garrison to perform what we considered to be their duty. We are now fully aware of one cause of that reluctance, besides another reason of silence mentioned by Mr Pym. It seems that the subject could not be touched upon without entering into all the disagreeable and vexatious circumstances attendant upon a medical controversy. Mr Pym has at last thrown down the gauntlet, or, what is the same thing, has taken up the gauntlet thrown down by Dr Bancroft and Dr Burnett, though they did not witness the Gibraltar disease; and they will find in him a more powerful adversary than any they have yet encountered. He comes into the field of battle so entrenched in official documents, and so confident of his individual strength, that no ordinary power will overcome him.

"The principal object which I have in view, is to counteract the influence which the opinions of Dr Bancroft have had upon the minds of the public as well as of the profession, by bringing forward evidence to prove, that it is a different disease from the bilious remitting fever of warm climates; that it is not produced by, nor in any way connected with marsh miasmata; that it has not been a constant resident in the West Indies; *that it is contagious*, and capable of being imported to, and propagated in any country enjoying a certain degree of heat; *that it attacks the human frame but once*, and attacks, in a comparatively mild form, natives of a warm climate, or Europeans whose constitutions have been assimilated to a warm climate by a residence of three or four years; and, lastly, that it differs from all other diseases, in having its contagious powers *increased by heat*, and *destroyed by cold*, or even by a free circulation of moderately *cool air*."

Mr Pym is of opinion that three very distinct diseases have been confounded under the name of *yellow fever*,—the bilious remittent fever,—the ardent continued fever,—and the contagious fever, to which he has given the name of the Bulam fever, because he considers it to be a specific disease, and has adopted Dr Chisholm's opinion, that it was imported into Grenada from the island of Bulam. Whether this be the real source of the disease or not, is of secondary importance; the name will serve the purpose of distinguishing it from the other fevers with which it has been confounded, if they be in reality different. The difficulty of the diagnosis lies in the rapidity of the progress of all fevers in warm climates, and in the skin in all of them being apt to acquire a yellow tint.

The existence of a sporadic continued fever in warm climates, caused by great heat, filth, and intemperance, independently of contagion or marsh miasmata, will probably be conceded by all parties. It will generally be recognized by its obvious connection with the cause, and by its not prevailing either among those communicating with the sick, or in the neighbourhood.

Nor will it admit of a doubt that marsh miasma is an extensive cause of disease in warm climates. But the diagnosis here is much more difficult. For we know nothing of the nature of marsh miasma, nor of the circumstances which constitute it a source of disease, and it is subject to many anomalies in its effects. Mere moisture is incapable of producing diseases originating from marsh miasmata, and the unhealthiness of a situation is by no means always such as from local circumstances we should anticipate. Besides their salubrity varies, as indeed might be expected, with the nature of the season; and the disease produced by miasmata will often remain latent for a great length of time. Again, the effects of marsh miasmata differ in different climates, so that the cold fever of Walcheren bears little resemblance to the bilious remittent of Hindostan. Still there seems to be some pathognomonic characters of marsh fevers. They have all a tendency to intermissions or remissions. Relapses are frequent, and having gone through the disease affords no protection against a subsequent attack. In cold climates, at least, it renders the constitution infinitely more susceptible of the cause. It is also generally connected with some degree of organic affection of the liver or spleen, so that the name of *ague cake* is given to the tumour and hardness observable in these organs. Marsh fever in its mode of appearing may be readily mistaken for the effects of contagion. It attacks about the same time many people inhabiting the same place, and of course communicating with each other; and as the cause spreads over a larger district, or continues operating for a greater length of time, the disease spreads from place to place. Also, new comers are particularly liable to be affected. The disease, however, is always endemical; and although it may lie dormant until the person who has been contaminated with the poison has been sometime removed from its source, in no instance does it spread from one individual to another, or affect any person who has not directly been exposed to the influence of marsh miasmata in some situation where they are generated. It generally returns in the same place with each return of the unhealthy seasons, and nearly in the same form; for although some variety in this respect may and does arise from peculiarity of seasons, as depending upon the degree of atmospheric heat, moisture, filth, &c. although it may appear for the first time from accidental floods, or be eradicated by proper draining of the marsh, still these causes are all obvious, and may be estimated.

Contagion is the grand subject of dispute, as the cause of disease in hot climates. It will not be contended with any plausibility, either that it never operates in such climates, or that it is the sole cause of fevers in these countries. The question is, if it be ever the source of any of those epidemics which are known by the name of *yellow-fever*. At first, it might seem easy to establish characters by which the contagious nature of any disease might be ascertained; but it is not so. Contagions vary very much in their properties, and in the nature of the diseases which they produce. They always, however, produce a specific disease, although this some times differs considerably in form, as in the case of Scarlatina simplex, and Angina maligna, and in the Proteus shapes of syphilis. They are also always communicated from one individual to another, by an effluvium, or matter generated by the disease. But the fact of this communication is not always easily proved; for many circumstances may counteract and prevent the effects of contagion, while, on the other hand, those to whom disease appears to be communicated, are commonly exposed to the same general causes which may have operated on those who are supposed to have infected them; so that, unless when the matter of contagion can be corked up in a bottle, or conveyed on the point of lancet, it is astonishing how difficult it is, as soon as scepticism is excited, to satisfy one's mind, whether a disease be contagious or not. Contagious diseases, however, generally proceed from one individual, and, after a short interval, spread gradually in all directions, independently of mere local vicinity, or similarity of situation; *and they* first disappear where they began. Diseases of situation and season, on the contrary, commonly affect several individuals nearly at the same time, who may have had no communication with each other, or appear at the same time in distant but similarly situated places; and they almost always remain longest where they began. Many contagious diseases do not affect the same individual oftener than once, others not oftener than once during the same epidemic. Besides some contagious diseases seem only to propagate themselves under certain conditions. Some require a certain temperature,—the contagion of typhus is rendered inert by heat, that of plague by cold. Some only affect the highly predisposed, others nearly all who are susceptible of the disease. Some require actual contact, others do not operate beyond a certain distance. Some act from a momentary application, others only after they have been applied for some time. Some act instantaneously, others may lie dormant an indefinite length of time. In these last respects, there are the greatest anomalies; and when we add the possibility of contagious diseases being generated from a coincidence of circumstances, the only way in which contagions can have been originally generated, and the possibility that contagion may be combined with or ingrafted, as it were, upon diseases not naturally contagious, we see most fruitful sources of interminable disputes. It may also be that some diseases are at one time really contagious, at another not contagious; and very often a disease may appear undoubtedly contagious, which is only endemic; or a really contagious disease may evince none of its characteristic signs. Contagionists have one great advantage over their adversaries. It is sufficient for them to prove the existence of contagion, in regard to the disease in question, in a single instance, to overturn the greatest mass of evidence on the opposite side. These general ideas are here thrown out with the haste of trimestral writers, with the hope that they may excite some attempts to a systematic history of contagion, its varieties, conditions, and laws; because we are convinced, that much disagreeable altercation would be saved, if these were previously established, and we had only to inquire whether any particular disease was possessed or not of those properties, which were held to be necessary and sufficient to constitute it contagious.

To return to Mr Pym, from whom we have been too long absent, it is necessary to mention that he is a contagionist, that our readers may be upon their guard against his natural bias, in observing occurrences, and relating observations. By *contagionist*, here, we mean to say that he believes the *yellow-fever* of the West Indies to be contagious, implying that he is disposed to admit as sufficient, what many others equally qualified to judge have rejected as insufficient, evidence to prove the existence of contagion in that particular disease.' Having premised this, we shall now let Mr Pym speak for himself.

"In the year 1794, I was surgeon to a flank battalion, commanded by the present Sir Eyre Coote, in the expedition under Sir Charles Grey, which landed in the island of Martinique on the 1st or 2d of February. It is unnecessary to describe the progress of the campaign; I shall therefore only observe, that, during the operations requisite for the reduction of the islands of Martinique, St Lucia, and Guadeloupe, the troops underwent very considerable fatigue, particularly the battalion to which I was attached, having in general been in advance, and taken part in storming all the principal heights in the different islands; and having been exposed, for nearly four months, to all the changes of climate at that season of the year; frequently bivouacing under temporary wigwams during the day, and, after long marches in the night, having nothing to protect them against the rains and damps but a blanket, which, in addition to a knapsack, every soldier carried upon his back.

"During all this time, however, the officers and men enjoyed much better health than they could have expected to have done under the same service in Europe. But after the surrender of Guadeloupe, when the troops employed in the capture of this island continued to enjoy the highest state of health, a dispatch was received by the Commander in Chief from Martinique, informing him that a very bad fever had broken out in the 70th Regiment, and that the surgeon had fallen an early victim to it. I received orders to repair to Martinique for the purpose of taking charge of the regiment; I immediately embarked on board a frigate, and in a few days reached my destination, (Fort Edward, close to the town of Fort Royal,) but not before 111 men had been carried off by it." p. 9, 10.

"From the manner in which the disease spread, I could have no doubt as to the existence of contagion. It originated among three companies quartered in some casements, or bomb-proof barracks, and was for sometime confined to them; the men in hospital, with other complaints were next attacked, and in succession, the surgeons and hospital attendants. It was some time before it was communicated to the troops quartered in the barracks upon a more elevated situation in the same fortification; but the moment it laid hold of them it commenced its ravages; while the only persons in the town of Fort Royal that suffered from the disease, were the officers who had joined the mess, or visited the sick officers of the 70th regiment.

"The first step I had recourse to, was to propose a change of quarters for the regiment, which was immediately ordered by General White, who then commanded. The men were encamped upon an airy elevated spot of ground, Point Negro, close to the sea, about a mile and a half distant from the town, where they experienced an almost instantaneous change for the better; after the third or fourth day, disease disappeared in the camp, where the regiment continued in the highest health for three weeks; before the expiration of which time, I was myself attacked with the disease, in its most violent form, and from which I recovered, contrary to the expectations of the medical gentlemen who attended me; but I was so much debilitated, that it was a considerable time before I was enabled to return to my duty, when I found the regiment removed to Case Pilot, a healthy village on the sea-shore, equidistant from St Pierre and Fort Royal; here they continued healthy until the arrival of the convalescents from hospital, with their knapsacks and blankets, which, being distributed among the different companies, communicated the disease so very generally, that, in the course of a very short time, it run through every individual in the regiment, with the exception of three officers, who, if they were attacked with the fever, had it in so mild a form, as to make it unnecessary for them to be confined to bed; and it is worthy of being remarked, that these officers had been several years in the West Indies, some time before.' p. 11; 12.

"For some months after the first breaking out of this disease, which was in March, we had no appearance of remitting, or intermitting fever, the disease run a continued course, in from one to three, or sometimes five days, never changing into, or terminating in ague, and although the convalescence was slow, particularly in the few who had survived venesection, the patients did not suffer in any one instance from visceral obstruction." p. 14.

So much for the West Indies: we shall now accompany Mr Pym for Europe. The Spanish fever first appeared, after an interval of at least thirty-six years, in Cadiz, in August 1800. For its history, we must refer to the report of the French commission sent to examine it, which was published in 1801, and which, with the usual allowances for human credulity and scepticism, we believe to be sufficiently correct. One part of the history of diseases of this kind we consider of great importance, as it is capable of being certainly ascertained, and as throwing great light on its nature, we mean the history of its progress from place to place, and the time of its appearance and decline in each. Such a disease, creating such alarm and havoc, is evidently unusual. It must either be the common endemial of the country, strangely increased in violence, or a new disease, generated by occasional circumstances, or propagated by contagion. In determining which of these it is, the history of its route and progress appears to us most important. If it proceed from the general state of the atmosphere, it should affect whole districts, or similar situations of whole districts nearly simultaneously. From a general cause, as heat or cold, dryness or moisture, the effects should be general upon the human constitution as it is upon vegetation. The common endemic of the country may certainly be so aggravated as to change its character, but it will be aggravated everywhere in an equal degree. The same is true in regard to the generation of new diseases from general causes; but if, in either case, local and limited causes operate, then the effect should be confined within the sphere of their operations, and either be stationary or migratory as the nature of the causes determined. But when we find a new disease, or at least a new form of disease, commence in one point of a particular place, spread gradually over that place, sparing others in the immediate vicinity; in another season leaving its original seat and attacking various places at some distance in different directions; then coming back to those places it had originally respected, and moving in this desultory way over a country for a number of successive seasons of various characters, we must either suppose that a pestilential vapour, of limited and defined extent, has been, during that period, hovering over the ill-fated country, and showering down destruction from time to time upon the places where it then might happen to be, or we must have recourse to contagion to explain its irregular progress.

The bilious remittent fever is a disease of many parts of Spain, returning annually at certain seasons, and the people are familiarized with the degree of mortality it produces; but it appears that a much greater mortality takes place from time to time at different places in the peninsula. During the present century it began in Cadiz, on the 10th of August 1800. On the 3d of September it prevailed in one of the suburbs of Seville, while the intervening villages and hamlets on the banks of Guadalquiver still remained healthy. Some time after it affected Xeres. From this time Spain continued healthy, except a slight attack in Medina Sidonia in 1801, until 1803, when the disease again shewed itself in Cadiz and Malaga. During the prevalence of the fever in other parts of Spain, Gibraltar continued healthy until the end of August 1804, when, for the first time since it came into our possession, a century ago, it was visited by a pestilential disease. Mr Pym was then absent, but he has inserted the following account of its introduction by Mr Kenning, surgeon to the Royal Artillery. This account, we know, has been objected to; but, for saying that is perfectly correct, we have the authority of Colonel Wright, who was then secretary to the governor, and drew up a most minute and able history of the disease, which, we hope, will not long remain unpublished.

"A shopkeeper named Santo (who resided in Boyd's buildings) arrived from Cadiz on the 28th of August 1804, and was taken ill on the 29th—he had lodged in a house at Cadiz, where some persons died of the then prevailing fever. Mrs Fenton (wife to Bombardier Fenton of the Royal Artillery) was the second person attacked; she was taken ill on the third of September; her husband and a child of the name of Roland, were taken ill on the 8th, and died on the 12th. Mrs Boyd, who had visited Mrs Fenton, was taken ill on the 13th, and died on the 19th; her husband was taken ill on the 14th, and died on the 16th; all those families were neighbours: the disease was confined, for some time, to this particular part of the town, and to those who had intercourse with them."

"An inhabitant of the name of Estrico (a neighbour of Santo's) alarmed at the mortality, moved his quarters to the south district, where he was taken ill, and from him the disease was communicated to his new neighbours, and rapidly to all the inhabitants. On the

> 12th of September, I visited every person then ill of the disease; they amounted to about 50. Most of them resided in Boyd's Buildings, the place where Santo was taken ill, and in ten of the families, I observed that more than one in each were affected. A meeting of medical gentlemen, civil and military, was assembled, by order of the Governor, on the 15th of September, when I pronounced the disease to be *malignant, and highly contagious.* I was the only person that ventured to give this opinion; and, standing single, I was of course overruled, as the highest medical authorities pledged their honour and reputation, that no other disease existed, than what prevailed there every year. The different medical officers were, in consequence, directed to visit all persons under its influence, to comfort and assure them, and others, that nothing was to be apprehended, and most particularly to prevent any idea of contagion being circulated abroad, as it would be prejudicial to the garrison." p. 20—22.

The judicial examination of Santo is in Colonel Wright's possession. However it originated, the disease spread rapidly, and apparently by contagion. Mr Pym says, that if an officer was taken ill, his servant was sent to hospital on the fourth day, and Colonel Wright has informed us that the second man of the 10th Regiment taken ill, was the bed-fellow of the first; and the second of the 54th supported the first to the hospital. In fact, the whole medical men of Gibraltar, with one exception, became convinced that the disease was contagious. So general a change of opinion must be received as an argument of considerable weight. The disease declined about the middle of November, as soon as the weather became cool. Out of 14000 inhabitants only 28 escaped, while out of 4200 military 1200 escaped. This marked difference will be variously explained by the abettors of different doctrines. Mr Pym ascribes it to the precaution enforced among the military of separating the healthy from the diseased.

> " And a most convincing proof that marsh had nothing to do with it is, that among all the civilians who emigrated from the garrison, and among the 1200 soldiers who escaped it, there never appeared, at any future period, any symptom of ague, or remitting fever, which Dr Bancroft has taken so much pains to prove may remain dormant in the system for many months." p. 39.

During the last four months of 1804, the mortality in Gibraltar was,—

" Officers - - -	54	Total 5946.
Soldiers - - -	864	
Soldiers' Wives and Children	164	
Civilians - - -	4864	

" Comparing this four months' mortality with the total number of deaths among the military, during the whole of the two preceding years, *viz*.

In 1802, Deaths - 35 } Total deaths in two years, 91,
1803, Deaths - 56 }

" And taking into consideration, that no such disease, during one hundred years, had ever existed there before, even in the years 1800 and 1803, when it prevailed both to the east and west of Gibraltar, viz. at Cadiz and Malaga, both places distant about forty-five or fifty miles, in a direct line, when, if any noxious change had existed in the atmosphere, we cannot suppose that it could have been confined to particular spots, or that a good police or quarantine regulation could have had any influence in altering it; for it was to this alone that Gibraltar, as well as many villages in Spain, escaped its baneful influence.

" Whether the system of purification carried into effect against the fomites of the disease of 1804, was absolutely necessary or not, it however proved successful in 1805, as the garrison enjoyed the most perfect health from that time until 1810. The occurrences of the last year, which I shall now mention, prove beyond a doubt the existence of a distinct and specifically contagious disease, which may be prevented, or rendered comparatively harmless, by the establishment of such precautionary measures as are had recourse to, with success, against plague, small-pox, &c.

" During the prevalence of the contagious fever at Carthagena in 1810, four transports from that port (two of them having on board deserters from the French army) anchored in the bay of Gibraltar, on the 19th of September; they were immediately put under the restraint of quarantine. I went alongside for the purpose of inquiring into the state of health of the men, when I was informed, that one was already dead, two were dangerously ill, and several others had been attacked with fever, which shewed itself in all the four transports in the course of a few days.

" Knowing the danger in which the garrison was placed, I made a representation to the Lieutenant-Governor, pressing him to order the ships to get under weigh for the Lazaretto at Minorca; and just as this was upon the point of being executed, information was received that the vessels would not be permitted to anchor there, the establishment of the old Lazaret having been given up, and the new one, which was then building, not being ready for the reception of passengers or goods. I then urged the necessity of procuring more vessels, for the purpose of separating and affording better accommodation to the troops, who were very much crowded. From unavoidable delays, these additional vessels were not procured so soon as could have been wished. At last, however, two hulks were appropriated for the purpose intended, but not before the disease had gained much ground, and many of the soldiers and sailors had suffered from it. Thirty-seven of the soldiers who had hitherto escaped it, were put on board one hulk, and forty on board the other.

"During the course of the first ten days, six men were removed to the hospital-ship from one hulk, and eight from the other; after which the disease disappeared, and sixty-three soldiers were saved from an attack of it, while all the sailors who remained on board, exposed to the contagion, were attacked with it. Mr Arthur, Assistant-Surgeon to the 4th Royal Veteran Battalion, who most humanely and courageously volunteered his services, was attacked with the disease on the fourth day after going on board, and from which he very narrowly escaped with life, having suffered most severely from that so often fatal symptom, the irritability of stomach."

"During the time that the disease had been going on on board the transports in the bay, the garrison continued in perfect health until the 20th of October, when, in consequence (as I must suppose) of a breach of quarantine regulations, which however could not be detected, a Minorcan family in the south district, belonging to the dock-yard, was attacked with the disease. The first information I received of it, was on the morning of the 26th of October, in consequence of the death of one of them, (a young man, clerk to Mr Boschetti); he had been pronounced convalescent on the 24th, having, as the medical person who attended him supposed, a remission of all his bad symptoms, which proved to be only a prelude to gangrene of the stomach.

"I immediately visited the family, and found, from the history of the case of the deceased, and the situation of the other members of the family and their friends, that the disease we had so much reason to dread actually existed. Six, out of seven persons, had been already attacked in one house, and three other families, neighbours who had visited them; as also, a Spanish priest who resided in town, at the distance of a mile and a half from the infected district, but who had visited, professionally, the brother of the deceased, a carpenter in the dock-yard, the first person taken ill, and who I suspected of having communicated with the infected transports in the Bay.

"I was aware, from what I had seen in 1804, that the only way of cutting short the contagion, was, by the most prompt and decisive measures, to separate the sick from the healthy, and to prevent any communication between those persons who might be suspected of having imbibed the disease, and the other inhabitants. I knew also that every art would be resorted to, with the view of avoiding quarantine regulations, if the necessity of having recourse to them should be made public. I communicated my opinion to the Lieutenant Governor, who gave me full powers, in concert with the Town Major, to take such measures as I might judge best.

"I directed a sufficient number of tents to be pitched outside the gates of the garrison, on the neutral ground, with a proportion of bedding, &c. and in the middle of the night, without any previous information being communicated to the inhabitants, a strong guard, with a sufficient number of carts, proceeded to the infected district, and conveyed the different individuals of the infected families, sick and well, with their baggage, to the encampment, which was established as a Lazaret, and kept in quarantine. I placed the sick in separate tents, and appointed persons to attend them who had had the disease at a former period. A cordon of troops was also established round the infected part of the south district, which was kept in quarantine for fourteen days; proper persons were also appointed to superintend the purification of the houses, furniture, &c. and to report the slightest appearance of disease among any of the inhabitants—who were also paraded, and inspected daily by one of the Hospital Staff.

"Within a few days, several persons (all neighbours of the first family taken ill,) were declared to be infected; particularly a servant of Mr Nichols, agent for transports. On the morning of the 28th, Mr Kidstone, surgeon, 7th Veteran Battalion, also reported to me the deaths of two men of that regiment, and that he had several other cases of fever in the hospital, which he suspected to be of the same nature;—five fresh cases of fever were also reported in the barracks, in the course of the day.

"I did not hesitate as to the measures which I ought to recommend, and in the course of next day the whole regiment, with the hospital establishment, was removed to the neutral ground. Ten soldiers who had the disease in the West Indies, were left in barracks for the purpose of whitewashing and fumigating them, and I recommended Mr Kidstone to employ the same description of men in attending the sick.

"Very few men were reported sick after the regiment moved into quarantine; three of them, however, were taken ill in the same tent; six died from the disease, *who were all taken ill in the same barrack-room.*" p. 46—52.

The last time the disease visited this garrison, was in September 1813.

"From queries put to Dr Gilpin by the members of the Medical Board, it appears that the individuals who brought the disease into the garrison were ascertained; one of them was ill when he arrived; the disease was communicated to the persons residing in the same house, and speedily on both sides of the street in which the house was situated. All those who cut off communication with the infected, escaped the disease; *of five hundred* persons confined to the dock-yard during all the time of the sickness, there was not an instance of one of them being attacked, although this was, of all others, the most likely spot for marsh effluvia to exist, and which, during the fever of 1804, suffered equally with other places, in consequence of the communication not having been cut off." p. 55, 56.

We have the authority of Colonel Wright for adding, "*while in a street, within thirty yards of the dock-yard, not a house escaped the fever.*"

"Upon the breaking out of the disease, nearly eight thousand persons left the garrison; the greater part of them were in huts or tents upon the neutral ground; there were very few cases of disease appeared among them, and those happened soon after their emigration, from the contagion being in the system. Upon the neutral ground there is a constant and strong breeze or current of air, sufficient to destroy the contagion of any disease, and in this way the contagion not spreading is to be accounted for this year, although, in 1804, when the sick were crowded into temporary barracks or sheds upon the same spot, all the hospital attendants were attacked by it." p. 57.

Long as this article already is, we must insist at considerable length upon another point, which we consider of very great importance, and which is now first distinctly stated, and seems to be established, by Mr Pym; and if we are right in our opinion, every one will agree with us, that he has deserved well of his country. He maintains, that the Bulam fever, we use the name only for distinction sake, affects the human constitution but once during life. We shall give the proof in his own words:—

"In my first visit to the hospitals, I found that all the orderly men and nurses had suffered from the disease, with the exception of the steward of the Queen's regiment, and the steward and nurse of the barrack artificer corps; the two last had been attached to the sick under my care in the West Indies, and my knowledge of this circumstance, I believe, first impressed upon my mind, the idea of the disease having a respect for the human frame, after having attacked it once. I communicated my sentiments to Sir Thomas Trigge, and requested him to order a parade of all the officers and men who had been in the West Indies, at which, to my astonishment and gratification, I found 122; many of them who had been exposed to the contagion in a variety of ways, but all escaping it, not one of them had had even the slightest headach." p. 25.

Twelve of the twenty-eight inhabitants of Gibraltar, who escaped the fever in 1804, had had it at a former period, either in the West Indies, Philadelphia, or Spain.

"It was now ascertained to a certainty, not only in Gibraltar, but by letters from Cadiz and Malaga, and by several persons escaping it who had had it at Philadelphia, that the disease attacked but once; all those who had passed it in 1800 and 1802, at Cadiz and Malaga, having been safe from its attack in 1804. Not that having been in the West Indies exempted them from an attack, unless they had passed the disease there." p. 28.

"The 57th regiment arrived at Gibraltar, during the prevalence of the malady, as a reinforcement to the garrison; it had been in the West Indies sometime before, and had a considerable number of men who belonged to it when quartered in the Island of Trinidad, and as their service on shore was very much wanted, they were allowed to land, and added to the proofs of the disease not attacking a second time." p. 29.

"I may also observe, that (in 1810) there was no instance of any person who had passed the disease before, being attacked with it, although several of this description visited and assisted the Minorcan family during the prevalence of the fever." p. 54.

Again, in 1813, Mr Fraser, deputy-inspector of hospitals, writes:

"Its specific character is still more distinctly marked, *by the exemption of all those during the present epidemic, who have formerly had a similar disease*, whilst former attacks of the bilious remittent afford no security; the disorder proving equally violent and unsparing to every class of persons, natives, acclimatés, or new comers, visiting every one within the walls who had not had it before, with the exception hitherto of about a hundred." p. 55.

"At the commencement of the disease, there were about five thousand persons within the walls who had had it at a former period, either in the West Indies, in Spain, or Gibraltar; and after careful inquiry, there did not appear to be one well authenticated instance of a person being infected a second time. The Spaniards are so fully convinced that the infection cannot be communicated a second time, that a certificate of their having passed the disease, is a sufficient passport through a cordon of troops into an infected town, which they enter without the smallest apprehension." p. 56.

The last proof, as well of the immunity of the constitution, after having passed through the disease, as of the identity of the Spanish and West India pestilential fever, is, from the letters of Mr Redmond, surgeon to the 54th regiment, which had gone from Gibraltar to Jamaica, and of Mr Rocket, deputy-inspector there. The former writes:—

"Having in my possession the names of the officers and men who had recovered from the Gibraltar fever of 1804, I was particularly attentive to ascertain if any of those would be attacked with the yellow continued fever of 1808; not a single case occurred, either then, or at any other time during the five years I was in that island. Latterly, men were selected from this list, as hospital attendants, which duties they performed with perfect safety to themselves; the single attendant above alluded to, and myself, being marked instances of escape under that head." p. 73.

To these proofs we have only to add, that Dr Rush was able to trace out only five cases of second infection in Philadelphia in 1797, three of whom were said to have had it in 1782, and two in 1741.

PART III.

MEDICAL INTELLIGENCE.

REPORT of DISEASES treated at the PUBLIC DISPENSARY, *near Carey-Street, London, from February 28th to May 31st* 1815.

ACUTE DISEASES.

	No. of Cases.		No. of Cases.
Tertiana	1	Peritonitis	1
Febris	29	Cholera	1
—— infantum remittens	5	Dysenteria	14
Otalgia	2	Hysteritis	3
Hydrocephalus	2	Rheumatismus acutus	11
Cynanche tonsillaris	9	Rubeola	5
—— trachealis	1	Variola	3
—— parotidæa	1	Scarlatina anginosa	7
Pleuritis	3	Erysipelas	1
Peripneumonia	13	Apoplexia	1
Catarrhus	101	Convulsio	1
Pertussis	10	Abortus	4
Hepatitis	3		

CHRONIC DISEASES.

	No. of Cases.		No. of Cases.
Vertigo	11	Icterus	4
Cephalæa	21	Hepatalgia	4
Hemicrania intermittens	1	Hypochondriasis	3
Catarrhus chronicus	31	Constipatio	5
Asthma	3	Diarrhœa	13
Hæmoptysis	25	Enterodynia	7
Phthisis	42	Hæmorrhois	4
Palpitatio	4	Tænia	2
Dysphagia	1	Ascarides	5
Dyspepsia	19	Lumbricus	1
Gastrodynia	22	Physconia	2
Hæmatemesis	2	Marasmus	10

Chronic Diseases continued.

	No. of cases.		o. of Cases.
Nephralgia	7	——— pericardii	1
Dysuria	2	Anasarca	6
Diabetes	1	Lichen	3
Hysteralgia	4	Prurigo	5
Dysmenorrhœa	3	Lepra	3
Amenorrhœa	7	Psoriasis	6
Menorrhagia	10	Urticaria	2
Leucorrhœa	17	Purpura	4
Rheumatismus chronicus	45	Erythema	3
Asthenia	39	Impetigo	4
Epilepsia	3	Porrigo	7
Chorea	1	Rupia	1
Hysteria	8	Eczema	3
Hemiplegia	4	Scabies	13
Paraplegia	1	Sycosis	1
Morbus spinalis	1	Ecthyma	1
Hydrops	4	Scrofula	2
Hydrops ovarii	1		

There has been nothing peculiar in the weather, during the three months comprehended by this report, except that March was warmer than April, and not infested by the easterly winds, which set in rather later, and have been less permanent than usual. The rains were frequent in May.

There has been nothing, in fact, either peculiar or prominent in the diseases which have occurred during this period. The usual majority of catarrhal and rheumatic complaints, sometimes single, but in very many instances combined, (the list only presents an account of the most prominent, where they occurred together) has been the principal subject of medical treatment. The fevers have been almost invariably mild, and many of them of a gastric or bilious character. In one instance only, where a poor woman and her two daughters were confined to the same room, without a bed, and destitute of proper nourishment, as well as of every other comfort, from temporary distress, all the three individuals became the subject of typhus, which, however, after they were removed to the Fever Hospital, was prevented from assuming a malignant type. The eruptive fevers appear, also, to have occurred only sporadically, or very partially, and, even in some very close courts, in which we witnessed them, to have shewn no considerable tendency to spread beyond the families in which they appeared.

The friends of humanity must be gratified to find, that some check is likely to be given to the indiscriminate inoculation of small pox, by the operation of the common law, which has been lately appealed to, and by which the punishment of imprisonment has been inflicted, by way of example, upon a woman, who exposed her inoculated infant in such way as to infect and destroy the lives of several other individuals; and also upon an unworthy member of the profession, who has spared no pains to multiply his own gains, by the dissemination of the pestilence through his own vicinity of the metropolis. "Sic utere tuo, ut alienum non lædas," is the principle of the law, as well as of common equity; and it will be favourable to the increasing security of the lower classes of the people, that their prejudices will have this wholesome restraint put upon them, and operating gradually, perhaps, to their final extinction. The act of inoculation is not unlawful; but if disease or death be, by that act, carelessly inflicted upon another, the law deems it punishable. We have lately had the satisfaction of seeing many instances, among the patients of the Dispensary, where vaccination was preferred; and perhaps the cessation of the violence both of opponents and advocates, aided by the public impression just alluded to, will gradually establish its general adoption.

London, May, 31, 1815. **T. B.**

Extract of a Letter from CASPAR WISTAR, *Professor of Anatomy in the University of Pennsylvania, to James Russell, Esq. Surgeon and Professor of Clinical Surgery, in the University of Edinburgh.*

During the last three or four years, various parts of the United States have been affected with a fever, which is marked by very extraordinary characters. In many instances, it is attended with a pneumonia; but even in those cases, as well as the others, in which there is no local inflammatory affection, the debility and prostration of the powers of life exceeds any thing we have ever witnessed before in this country. The disease has often terminated fatally in less than twenty-four hours from its commencement, with the symptoms of malignant fever. It has been most successfully treated by sweating from the commencement of the disease, and by the most profuse exhibition of stimuli, of almost every kind. General bleeding is of course a very hazardous remedy. In some places, the local affection is in the throat, resembling cynanche tonsillaris. It has extended from the northernmost parts of the United States, into North Carolina; and from the sea coast to the west of the Ohio River. It has also been in Canada. Warm weather, as in some other cases of typhus, seems to suspend its progress; and happily it does not spread much in large towns, having been in Boston, New York, and Philadelphia, without doing much damage in either of them. In this respect, as well as its prevalence in winter, it appears to be essentially different from yellow-fever.

Philadelphia, April 11, 1815.

Extract from a Letter on the best mode of softening Adhesive Straps.

THE custom of carrying up *chauffres* with cinders in them, still in use in some hospitals, is highly objectionable, both on account of the offensive nature of the vapour to the patients, and of their answering their purpose imperfectly. All that is necessary, and what is used in most English hospitals, is a large tinned case, from a foot to a foot and a half in diameter, filled with hot water. The back of the strap is applied around the circumference of the case, and is retained there till the plaster be as much softened as you wish. The greatest advantage of this method of warming the plaster is, that it produces an equable heat, and at the same instant of time; of great consequence, particularly after amputations.

Dr PINCKARD is preparing, for speedy Publication, a new Edition of the " Notes on the West Indies," in two volumes; with Remarks on the Condition and Treatment of the Slaves, and additional Observations on Martinique, Jamaica, and St Domingo, including Hints concerning the Seasoning or Yellow Fever of those Colonies.

THEATRE of ANATOMY, Great Windmill Street.

The Winter Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, by JAMES WILSON, F. R. S. and CHARLES BELL, F. R. S. E. Surgeon to the Middlesex Hospital, will commence on Monday, the 2d of October, at Two o'clock.

The Demonstration in the Anatomical Rooms will be given by Mr SHAW during the forenoon.

Sir EVERARD HOME will deliver Twelve Lectures on the Operations of Surgery to the Pupils of St George's Hospital.

Two Courses of Surgical Lectures will be delivered by Mr CHARLES BELL during the Season. These Lectures will be given in the evenings of Tuesday, Thursday, and Saturday, at seven o'clock. The first division of the Course will embrace the principles of Surgery, illustrated by the preparations in the Museum, and the practice of the Hospital. The second division will consist of the Operations of Surgery, performed on the dead body.

Once a week during the Course, a Review will be made of the Occurrences and Practice of the Hospital.

Medical and Chemical Lectures, St George's Hospital, and George Street, Hanover Square.

These Lectures will recommence in George Street, Hanover Square, the first Monday of October, at the former Morning hours, by Dr PEARSON and Dr RICHARD HARRISON.

Communications have been received from Mr ESTLIN, Mr LYON, Mr HEATH, Dr REED, Dr LITHGOW, Mr DUNCAN, and Mr FLETCHER.

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MEDICAL AND SURGICAL JOURNAL.

1. OCTOBER 1815.

PART I.

ORIGINAL COMMUNICATIONS.

I.

Cases of Aneurism. By JOHN MACKESY, Surgeon 1st Battalion 62d Regiment.

CASE I.

VINCENZO GELARDI, ætatis 38, a native of Palermo, by trade a whitesmith, found labouring under an aneurism of the femoral artery, of an immense size.

About a year from the period of my seeing him, while in the act of lifting a heavy forge-bellows, he suddenly felt a violent pain, and sense of tearing, on the inside of the right thigh, at the part where the artery perforates the tendon of the triceps: he immediately lost the power of supporting himself, and fell on the floor in great agony. On recovering from the first sensation, he examined the pained part, and discovered, that a pulsating tumour, about half the size of an egg, had already formed. On the tumour increasing in size, he was admitted into the principal hospital of Palermo, two months from the date of the accident; some months after he was removed from the chief hospital to that of Sant' Bartolemeo, an hospital appropriated to chronic, and what are deemed incurable complaints.

I saw him on the 7th October 1813; the disease had now existed twelve months. He was lying in the ward of incurables, and in a manner abandoned to his fate. Although great emaciation had evidently taken place, the patient's constitution appeared tolerably good, and the pulse was not much accelerated beyond the healthy standard. The affected limb was covered with a dirty piece of linen, wet with some acid embrocation; the natural outlines of the thigh were much changed; its inside, from within three inches of Poupart's ligament to the inner condyle of the femur, was occupied by a large pulsating tumour; the part of which next the knee was of a black, or rather a dark livid colour, and its most elevated point hard, cold, and incompressible; fluctuation could be distinctly felt under the livid part, and from the apparent thin and diseased state of the integuments, their rupture at this point might be daily expected; the leg of the affected limb was considerably wasted, and some degree of œdematous swelling existed in the foot and instep.

The gentleman in whose service the accident happened, begged of me, if possible, to afford the unfortunate man assistance, representing, at the same time, that the surgeons and physicians of the city had previously declared the impossibility of arresting the impending danger. I told him, that although, from the advanced stage of the disease, the expedient would be doubtful, however, that an immediate operation might present some hopes of saving him from inevitable destruction.

On this opinion being intimated to the patient, he immediately gave his consent to the performance of the operation, rejoicing to find that any hopes of safety yet remained.

To give a fair chance to the operation, under such critical circumstances, it became necessary that the patient should be placed under my own care; and to effect this, I obtained permission to remove him into a small unoccupied ward in the Detachment-Hospital, then under my direction.

The operation was difficult, the knee of the affected limb having been kept for many months continually bent, from the patient finding that the easiest position; the leg formed an acute angle with the thigh; and, owing to the same cause, the thigh made an obtuse angle with the pelvis, thus forming a hollow immediately below Poupart's ligament somewhat resembling an axilla, which increased the difficulty of dissecting for the artery; the base of the tumour also extending so near the femoral ligament, rendered the application of the tourniquet impossible; and the small space at the upper part of the thigh, which was not included in the base of the tumour, partook so much of the general tumefaction, that the pulsation of the artery could not be felt, so as to be of use in directing the first steps of the operation.

On the morning of the 8th of October, the operation was commenced by bringing the affected thigh into as direct a line with the pelvis as the situation of the limb would admit; a free incision, of about five inches in length, was then made through the integuments, commencing a little above Poupart's ligament, and continuing it down in the supposed track of the femoral artery, over the upper part of the base of the tumour.

On dividing the integuments, a feeble pulsation of the artery could be felt at the upper part of the incision. On a few more strokes of the knife, a diseased mass of condensed cellular substance; of glands preternaturally enlarged; and of thickened fasciæ, were brought into view. The further steps of the operation were in some degree impeded by the blood thrown out by some twigs of the glandular arteries preternaturally enlarged. A cautious and tedious dissection at length laid bare the condensed cellular substance immediately including the artery, which being carefully opened, and slit up for some space, the artery was without difficulty separated sufficiently from its attachments, with the handle of the scalpel; and, by means of the common eye-probe, a double ligature being introduced under it, one of them was tied two inches below Poupart's ligament, and the under one a little lower down, leaving a space of half an inch between them. The artery was now divided between the ligatures, and allowed to retreat within its surrounding cellular substance.

The wound appeared now of a great depth, owing to the tumefaction of the part; and the course of the artery was somewhat removed from its natural direction, being forced more inward, by the vicinity of the tumour. In prosecuting the dissection, the utmost caution became necessary not to open the fascia at the lower point of the wound, as here it appeared to form the immediate covering of the superior part of the aneurismal tumour. The wound being cleared of blood, the integuments were accurately brought together, and secured by sutures and adhesive straps.

The poor man bore the operation admirably, and his joy, on being informed that it was over, became so great, as to render it necessary to moderate it, by assuring him he was still in the utmost danger. The wound being dressed, the tumour was now examined; all pulsation had ceased; in other respects, the same as before. The patient, being laid in a bed made in the operating-room, was left to repose.

9th October, morning visit.—Rested tolerably well during the night; pulse very little accelerated, and his skin of a natural temperature; the surface of the tumour was warmer than usual, and the foot and leg of the same temperature as the sound one. Low diet, and no medicines.

10th.—The patient costive. Habeat ℥i. sulphat. sodæ; the apex of the tumour softer, and some slight diminution of its volume was remarked; apyrexia.

From the 11th to the 17th October, the wound daily dressed; discharge considerable, and of a healthy consistence; the tumour evidently diminishing; the patient is evidently improving in appearance, with good appetite, and free from any bad symptoms.

The patient went on progressively improving in health and spirits, until the 5th of November; the wound at this period was almost entirely closed; and the aneurismal tumour reduced three and a half inches in its circumference. At this period, several cases of bilious remittent fevers occurred in the garrison; a thick clouded atmosphere, with a scirocco wind, prevailed for several days, succeeded by frequent and sudden variations of atmospheric temperature.

On the evening of the 6th of November, the patient was seized with an attack of fever, ushered in by cold rigors; and on the morning of the 7th, he laboured under quick pulse, increased heat of the surface, foul tongue, loss of appetite, with a sense of oppressive weight in the epigastrium. The aneurismal tumour had also undergone a remarkable change. From being tense and circumscribed on the inside of the thigh, and having its surface of the natural colour, it had, in the short space of 24 or 30 hours, assumed a soft, flabby, unresisting feel, and its surface appeared red, and inflamed; the swelling of the limb became more diffused than usual, the whole thigh partaking in the œdematous tumefaction. The wound, although previously almost healed, reopened in part, and discharged a thin watery serum.

He was ordered a weak solution of tartrite of antimony in repeated doses, till full vomiting occurred; the wound was dressed in the usual manner

8th November.—The febrile symptoms somewhat abated; the general appearances of the thigh and wound nearly the same as yesterday; the livid coloured integuments, near the inner condyle, are become much softer; and sloughing at that point is about to take place. A lancet was struck into the tumour at this part, but the presence of a placental mass of clotted blood underneath, prevented the exit of much fluid, a little thin brownish serum only issuing from the puncture.

The patient being costive, had a few grains of calomel and jalap.

9th.—The febrile symptoms continue; he passed by stool two large lumbrici; a livid vesicule now appeared on the upper part of the base of the tumour, nearly where the lower point of the first incision terminated; the lips of the wound that were united have opened out anew; the discharge thin and watery.

10th.—Two or three more livid points have formed on the apex of the tumour; the fever has now assumed the low remitting type, with evening exacerbations.

11th.—At this morning's dressing sloughing took place at the livid points, leaving holes in the covering of the sac, about the size of a shilling, through which the black clotted contents of the tumour protruded; the constitutional symptoms continued, with quick pulse, yellow foul tongue, and sunk cadaverous countenance.

Considering, under these unfavourable circumstances, that all hopes of the contents of the tumour being absorbed were at an end, it was deemed most advisable to evacuate immediately the entire contents of the sac; and thus by removing an enormous morbid mass, relieve the constitution, and prevent further destruction of the surrounding parts. The sac being therefore laid open, in the direction of the several openings already formed along the apex of the tumour, large masses of clotted blood were rolled out with the fingers, and with a spoon, towards the bottom of the sac; the contents were found of a pale yellowish colour, of a firm consistence, formed of the compressed fibrin of the blood, and apparently composed of short tawny fibres, arranged in longitudinal layers.

That portion of the contents, which immediately occupied the bottom of the sac, retained, on its being removed, the impression received from the shaft of the femur; the contents of the sac filled nearly a large bason, and amounted to about four pounds; the cavity was now freed from all extraneous matter, with warm water.

The inside of the affected thigh now presented immense loose folds of discoloured integuments, enveloping a deep cavity, which extended from Poupart's ligament to the condyle of the femur.

The os femoris could be felt at the bottom of the sac, for the space of five or six inches, free from caries, and covered only with its periosteum, which was ragged, and of a diseased brownish colour.

The contact of the contents of the tumour, with the long continued pressure, had altered the relative situation and natural appearances of all the surrounding parts; a few loose discoloured shreds of thickened fascia, with the tendinous insertions of the triceps, could only be recognized. The greater part of the triceps, vastus internus, and Sartorius, were either destroyed by disease, or pressed into union, and formed a confused mass with the covering of the tumour; the continuation of the artery towards the ham was not perceived.

The cavity of the sac was now plentifully strewed with charcoal finely powdered, and the integuments brought together, and retained by the many-tailed bandage; the patient washed, linen changed, and laid in another bed.

12th.—He slept somewhat during the night; feels better this morning; the discharge very considerable; the charcoal has removed all disagreeable fetor; the cavity dressed as before, and the loose folds of the sac supported with a roller.

20th.—All alarm from constitutional symptoms of disease has now subsided; a slight febrile accession occurred every evening for seven or eight days, but has now ceased; healthy granulations are rapidly forming in the bottom and sides of the sac.

A minute detail of daily circumstances would prove tedious and unnecessary; the integuments were supported at each dressing with adhesive straps, and well applied rollers; the sides of the sac have lost the thin leather-like consistence, and are becoming more firm and fleshy; the sore is filling up from the bottom. A moderate diarrhœa, for the first few days after the evacuation of the sac, rendered small doses of opiates necessary; little of any other medicines was exhibited. He had a pint of wine daily, and half diet; his health and spirits are improved, and he occasionally rises from bed, and supports himself on crutches for a few minutes.

December 29.—The remaining sore is now trifling, and the discharge so inconsiderable as only to require dressing every third day; he can extend the leg of the diseased limb nearly on a line with the thigh; and, in walking with crutches, he can place the toes and part of the foot on the ground. The inside of the thigh, where the aneurism was situated, is assuming some degree of its natural fulness.

About this period he was removed to the city hospital in a state of convalescence, the sore not being completely healed.

Case II.

Francisco di Giovanne, ætatis 50, a native of Catania, in Sicily, by trade a brazier, has had for many years previously, retention of urine; the origin of which complaint he attributes to his receiving cold, while labouring under gonorrhœa. He was in the habit of passing small calculi and sand with his urine, attended with pain in the perinæum.

In the month of October 1812, while endeavouring to raise a heavy copper boiler, he felt something tear, or give way, in the right groin, attended with extreme pain. On examining the pained part, he perceived a small pulsating tumour had formed, about the size of an almond, which gradually increased in size; its advance, he conceives, has been considerably forwarded by his straining to make water.

I saw him, on the 15th of October 1813, in a ward of the chief hospital of Palermo, and the following appearances were observable:—A large oblong pulsating tumour occupied the right groin and inside of the thigh; its base extended from the spinous process of the ilium to within six inches of the knee; its apex rather on the front of the thigh; and, about four inches below Poupart's ligament, a turgid branch of a vein run along its surface, which was painful, and covered with an erysipelatous blush of inflammation. The pulsation of the tumour was so strong, as to render the raising and falling of the bedcloths perceptible at each stroke of the artery; the foot and leg of the affected limb were œdematous, but of a natural temperature; the pulsation was most distinctly felt immediately under the femoral ligament, and at this point each pulsation gave to the finger a sort of jarring sensation; a low irritable fever was present, with quick wire-like pulse, 126 in the minute; great emaciation had evidently occurred, which, with a sunk eye, and general cadaverous aspect, forcibly declared his miserable situation.

His medical attendants, viewing him, as it were, already in the grave, ceased their unavailing attendance. Extreme unction has been four times administered to him; and he has daily at his bed-side a priest, preparing him for another world.

The case was clearly an aneurism of the femoral artery, the vessel being ruptured where it passes over the brim of the pelvis, and under the ligament of Poupart. The complaint was of twelve months duration, and was deemed by the attending medical officers of the establishment as incurable; the patient was of course left to a certain fate.

I proposed to one of the principal physicians of the hospital, (Dr Calcagni) that there still existed a possibility of saving the patient by an operation, although from his advanced age, and unfavourable situations, the result would be very doubtful. This gentleman, in concurrence with the sub-director of the hospital, most readily agreed to an expedient which even held out remote hopes of success. The patient himself most anxiously begged that the operation might be performed immediately.

Permission being obtained for his removal into the British detachment hospital, he was removed there on the evening of the 16th of October 1813. On the morning of the 17th the operation was to be performed, but we were obliged to defer it till next day, in consequence of the height of the fever, and extreme agitation of the patient.

From the ease derived from a relaxed state of the muscles, the patient, for months before, kept the thigh bent towards the abdomen; the increasing size of the tumour fixed it in this position, so that at this period the thigh could not be stretched out on a line with the pelvis. The superior spine of the ilium could not be distinctly felt, in consequence of the base of the tumour extending over it. The ligament of Poupart was pressed upwards, and considerably elevated, crossing like a band the upper part of the tumour.

18th of October.—The patient being now more tranquil, although the fever continued, with a pulse of 120 in the minute, further delay was deemed hazardous. Being favoured with the very obliging assistance of Surgeons Shortt, Prichard, and Boltan, the operation was commenced in presence of a numerous body of the city surgeons and physicians.

An incision was made through the integuments, commencing two inches above the superior spinous process of the ilium, and about one and a half inch from it, and terminating at that point of Poupart's ligament, where the pulsation appeared most distinct; its direction downwards was sufficiently removed from the abdominal ring, as to pass on the outside of its superior internal opening, thus avoiding the hazard of wounding the spermatic chord, or epigastric artery. The first incision brought into view the cellular substance covering the tendon of the obliquus externus; this substance was much firmer than natural, assuming the appearance of a yellow tendinous expansion. This, with the tendon of the oblique muscle, being dissected through, and slit down to the ligament, the internal oblique was laid bare.

From the morbid thickness which the parts had acquired, it became necessary to divide some fibres, before the finger could be admitted between Poupart's ligament, and the edges of the obliquus internus and transversalis. This being carefully done, I was now enabled to introduce the fore-finger of the left hand between these muscles and the peritonæum, which formed a director to the bistoury, with which I divided them in the direction, and nearly to the extent, of the external wound.

From the flaccid state of the abdomen, owing to the emaciation of the patient, it only became necessary to disturb the peritonæum in a slight degree; but the access to the external iliac artery was impeded by a confused mass of glands and cellular substance. These being separated, and held aside, the artery was discovered free of the tumour, and pressed inwards on the psoas parvus. Some fibres of the fascia iliaca, where it formed a sheath to the crural vessels, were divided between the nerve and artery. With the handle of the scalpel, the artery was now sufficiently detached. A blunt needle, with a double ligature, was, without difficulty, passed beneath it. Each ligature being tied, the artery was divided between them, about two and a half inches above Poupart's ligament. On the artery being tied, all pulsation ceased in the tumour; its surface, a moment before red and inflamed, became immediately pale, with diminution of temperature; the entire limb likewise became colder than usual a few moments after. The integuments were now brought together, and retained by sutures and adhesive plaster. A bed being made in the room where the operation was performed, he was placed in it, and left in charge of a careful attendant.

18th and 19th.—The aneurismal tumour not so elevated; its surface is no longer inflamed, and is assuming the appearance and feel of healthy integuments. The pulse is still quick and small, but he is free from any considerable febrile symptoms; feels appetite; a slight discharge of serum from the wound; costive; ordered an ounce of the sulphate of magnesia.

On the 20th, the dressings were removed; the discharge thin, but not unusually great; the wound of a healthy aspect; no untoward symptom; appetite good; on low diet, with a pint of beef-tea daily.

From a very few hours after the operation, there has been no diminution of heat, or perceptible interruption to the circulation in the limbs; the œdematous swelling still remains in the foot. On the 11th day both ligatures came away.

December the 6th, 48 days from the date of the operation, the wound was almost cicatrized; the patient has acquired flesh, and has lost that cadaverous appearance, so striking at the first view; all symptoms of pyrexia ceased on the fifth or sixth day from the operation; his general health is much improved.

Absorption is rapidly taking place in the tumour; its size is considerably diminished, and its surface cannot be distinguished from healthy integuments; he can extend the diseased limb nearly on a line with the pelvis; he has been on crutches the last fortnight.

17th.—The wound is now entirely healed.

22d.—Two months from the period of the operation, this man was discharged from hospital perfectly recovered; the tumour is almost entirely absorbed; he walked home to a distant part of the city with the assistance of crutches.

Previous to my departure from Sicily, I had the pleasing satisfaction to learn, that he has recommenced his trade, and supports a wife and family, which were reduced to beggary by his long protracted illness.

II.

Case of Amaurosis cured by active Treatment. By JOHN BISHOP ESTLIN, Member of the Royal Medical Society, Edinburgh, and of the Royal College of Surgeons, London.

THE following case of amaurosis having terminated very favourably under a plan of active treatment, I am induced to send it to you for publication, in the hope that it may excite other practitioners to adopt similar measures, before they pronounce these alarming attacks to be of a hopeless nature.

On Tuesday, the 14th of last February, Mary Thomas, aged eight years, was brought to me, by her parents account, quite *blind*. She had been at school, and was employed in sewing and reading on the preceding Thursday, and had made no complaint of any defect in vision, or of unusual indisposition. During the night of Thursday she slept well, but upon awaking after daylight on the following morning, she asked for a candle to enable her to dress herself, and the parents discovered that she was incapable of seeing. Upon questioning her minutely since, I learn that she had at that time some perception of light, but not of objects; of the latter I was able to judge myself by seeing her walk about the room she was accustomed to live in, five days after the attack, as she could then only ascertain the presence of any thing in her way by feeling with her hands.

By inquiring of the parents of the child respecting the prevailing disorders in their family, I found that they had lost five children by some disease of the head, of which the most striking symptom to their observation was convulsions. The mother recollected, when I particularly asked her, that the subject of these remarks had for some time complained of headach, though it was not sufficient to prevent her from attending regularly the Lancastrian girls' school in this city, of which she was a pupil. Her bowels were in general rather constipated. These circumstances, together with the severity of the attack, made me apprehensive that the optic nerves were affected with a greater degree of disease or pressure than it was in the power of medical treatment to relieve.

On examining the eyes, the pupils were found considerably dilated, the left most so; and in neither iris was there the slightest action from the stimulus of light. She could see the light, and the reflection from the face of a watch. Her appetite had been good since the attack, and she manifested no general indisposition. No paralysis had affected any muscle. The pulse was rather full, but not quick.

I immediately took away a little more than six ounces of blood from the arm; vomiting and fainting were produced by the bleeding. A powder, with four grains of calomel and sixteen of powder of jalap, was given in the afternoon, and ordered to be repeated in the morning. The head was shaved, and a blister applied to it.

February 15th.—The powder last night was rejected, but that taken this morning was retained, and has produced one evacuation. The blister has not yet acted. Her sight is a little improved; the pupil of both eyes is not quite so much dilated, and the left acts slightly upon the sudden admission of light. The same powder ordered to be repeated to-morrow morning.

16th.—She feels much headach and is very drowsy, though her parents fancy she can see a little better. Pulse full, not very quick. I took away eight ounces of blood from the arm. Fainting came on while the blood was flowing, attended with vomiting and copious relaxation of the bowels. The stools have no particular appearance.

17th.—The sight of the right eye is considerably improved; she can perceive with it most objects that are of a light colour. The left pupil is more contracted, and the iris has some action when the degree of light is varied. The left pupil does not contract and dilate to-day so readily as it did yesterday. The degree of sight, however, in the left eye, does not appear to be less than it was. She cannot distinguish colours; red, she calls purple. The bowels have been freely opened, and she has no pain in the head. She feels lively and comfortable. She was directed to take a powder with three grains of calomel and 12 of jalap, night and morning.

18th.—Only one evacuation. She feels well. Left pupil more contracted; she still sees best with the right eye, and was able to-day to distinguish the hands of a watch. To continue the powders.

19th.—One evacuation only. To take this evening and to-morrow morning five grains of calomel, and 20 of jalap.

21st.—She has had several stools. Sight rather improving. The iris of both eyes more active. Her gums are affected by the calomel, and her teeth aching. The blistered surface upon the scalp is discharging by means of the savine ointment. To omit the powders, and to take night and morning ʒii. of Epsom salts.

23d.—Salts operate well. Mouth still sore. The pupils are more contracted. She can point to the figures upon a watch, and can see large letters. To continue the salts.

This plan was persevered in, with the addition of a quarter of a grain of the tartarized antimony twice a day, and she was directed to hold her eyes frequently over some aqua ammoniæ.

On the 1st of March she was able to read letters of a middling size. The pupils were contracted to a natural size. The irises not very active. The blister dry. Bowels open. The aqua ammoniæ produces much watering of the eyes.

March 5th.—She can read small print. To omit the tartarized antimony; to continue the salts once a day, and the application of the aqua ammoniæ twice.

On the 6th of March, 25 days from the attack of the disorder, and 21 after medical treatment had been resorted to, she returned to school, and has continued her usual employments of reading, writing, and sewing, without any inconvenience.

The little girl is now cutting the second set of teeth; I have lanced her gums, and removed some decayed teeth. I conceive she will require attention for some time, to prevent any irritation, particularly near the brain. She takes once or twice a week a doze of Epsom salts, and I have made an issue in her arm.

What would have been the result of this case had not remedies been employed, I cannot take upon myself to determine; but I think it not improbable, that the loss of sight might have been permanent.

Similar instances of recovery may have occurred in the practice of other professional men, though none have very recently been laid before the public. If the statement of the above case lead to the successful trial of the same measures in a single example of gutta serena, instead of its being pronounced irrecoverable, as this was considered by others who saw it, my object in its publication will be very fully answered.

Bristol, May 30, 1815.
22, *College Green.*

III.

A Case of Hydrophobia, unsucessfully treated by copious Bleeding.

By Dr Albers of Bremen.

The very interesting case of hydrophobia, stated by Mr Marshall, inserted in your Journal, No. 37. p. 26. 30th January 1814, and wherein also two cases of the hydrophobia, cured by copious bleeding, are quoted from the Madras papers, have induced me to try this copious bleeding, though as ineffectually as in Mr Marshall's case, upon a girl afflicted with the said terrible disease. I do not in the least mean by this to call in question the efficacy of this remedy, which already has been recommended by the most ancient physicians; for it were impossible to deny the successful experience of Grisley, Hartley, Hillary, Peters, Poupart, and many more. My principal motive for handing you the present statement, and my requesting the insertion of it in your Journal, is to prove, that copious bleeding, though applied at an early period, does not even always protract the course of the disease; since the cases, where the patients died within 24 hours, as happened in my case, and of which Morgagni (Ep. viii. Art. 22.) gives us an instance, are to be numbered among the rare occurrences. In the very first case that might happen to occur to me, I should again have recourse to the same remedy; since at Vienna and in Saxony I saw several hydrophobic patients die, with whom different methods were tried; and since it is proved by the interesting statement of Dr Michael Gherardini, * that, after the most different and opposite treatments, they generally perished, so that, in the present state of the medical art, we may look upon the cured as mere exceptions.

Sophia Sturmann, 34 years old, on the 4th of November 1814, happened to pass by two dogs fighting with each other. One of them rushed upon her, and bit her in the carpal part of the right hand, on which occasion only the skin was injured, and immediately ran off, so that it remained dubious whether the dog was actually mad or not. She immediately repaired to a surgeon, who, however, in the opinion that, in that season of the year, there were no mad dogs, strove to tranquillize her on the occasion, and dressed the wound only superficially, and in the common way. But as in the evening she was told that a mad dog, probably the same, had been seen in that part of the town where she had received the injury, she did not rest on the assertion of the surgeon, and the very same night, at half past eleven, fourteen hours after the bite, repaired to Dr Olbers, who at once considered the case as highly critical, ordered her the use of the belladonna, and bid her repair the very same evening to the surgeon, whom he directed to scarify the wounds as deep as the tendinous structure of the parts, thereabouts situated, would permit, to elicit as much blood from them as possible, then to wash them out with salt-water, and bring on an inflammation and suppuration, by strewing them over with the pulvis cantharidum; he also ordered the patient to rub in, on the following days, two drachms of unguentum mercuriale, so as to bring on a salivation. In how far, and how soon, these directions of Dr Olbers were put into execution, he never learned, since the person bit did not call on him but only once more, and about eight or ten days posterior to the bite. So much, however, came to his knowledge, that the rubbing in of the unguentum mercuriale was neglected. The belladonna, consequently, is the only remedy, of which, with a certainty, it can be said that it was used, and this only at a later period, in a dose of six grains morning and evening.

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* Osservazioni medico-pratiche sulla cura della rabbia, fatte nello Spedal Maggiore di Milano, da Michele Gherardini, medico dello stesso Spedale, e di quello della Senavra. Vid. Giornale per servire alla storia ragionata della medicina di questo secolo. Tom. V. pag. 1. seq.

Three weeks afterwards, Dr Olbers requested me to visit the patient for him. She had on that night experienced the first symptoms of the hydrophobia. I found the patient, in appearance a stout robust girl, in her bed. She then complained of a violent pain in the injured right hand, the whole arm, and the right side of the face; which pain she had already felt a couple of days before, though less violent; so that, two days before, she had walked to a place at the distance of about two English miles from town. She thought that these parts so affected were also swelled; which, however, I could not see. She further complained of a violent and lasting anxiety, so that she had not a moment's rest anywhere, nor was capable of swallowing anything liquid, though she was tormented by the most parching thirst, because she then felt as if her throat was strangled, and she was going to be choked. On my entreating her to swallow some water-gruel, with her eyes closed, she indeed attempted it; but scarcely had she approached the cup to her lips, and taken a few sips, than she experienced a violent anxiety, so that I was compelled immediately to remove the vessel. She had a clear foresight of her perilous situation, and no arguments nor consolation of mind could tranquillize her on that score. It was then nine o'clock in the morning, and scarcely six hours after the appearance of the first symptoms of the hydrophobia. I directed the surgeon, who attended me, to draw nearly forty ounces of blood from the vein of the right arm, on which the patient assured me she felt some relief. She could see the blood run out, without experiencing any anxiety, nor did the transfusion of water from one vessel into another cause her any disagreeable sensation. Internally I prescribed three grains of calomel, with one scruple of sugar, to be taken every two hours. At five in the afternoon I saw her again, when her anxiety and uneasiness were much more violent than in the morning, and were expressed in the whole face, and in her eyes wildly rolling. The patient had till then regularly taken the pills, but now positively declared she was not capable of getting down any liquid; upon which I prescribed the same dose of calomel in the shape of pills. Her pulse now was feverish, quick, and rather full, the heat of the body considerably increased. I, upon the spot, and in my presence, ordered a second evacuation of blood of about forty ounces. When the blood, issuing from the vein, happened to touch the hand of the patient, she felt violently frightened, and her whole body experienced slight convulsive tremors. As the patient lived in a very small habitation, and was obliged to sleep, with three other females, in a confined and narrow room, I advised her to have herself carried to the Infirmary of this town, being thoroughly convinced that the requisite remedies might be much more conveniently applied there. This she peremptorily refused, and requested me to procure her, instead of that, some other abode, because she thought she could then die more tranquilly, well aware she could no longer be saved. At nine o'clock in the evening she was transferred thither, and two hours later I paid her a visit. I found her sitting by the stove, and by deep sighs evincing the great internal anxiety of which she constantly complained. In spite of her excruciating thirst, she could not swallow a single drop; and the word *drink* alone was now sufficient to produce much more violent convulsive tremors in all her frame. The pills and some bread she swallowed without any difficulty. The pulse was quick, yet not so low as to make me think I durst not venture on a repeated bleeding. When twenty ounces of blood had been drawn from her right arm, she nearly fainted, but soon recovered, and was put to bed. Soon after I had withdrawn, at eleven o'clock, she experienced a still greater oppression and anxiety, started from the bed, ran to and fro in the room, and from time to time cried out so, that her cries were heard at the distance of several houses from her habitation. She repeatedly warned her attendant to keep at a distance, that she might not bite him. She said that she was an unfortunate creature, and did not wish to bring her misfortune upon others. I am still uncertain whether she actually felt this propensity to bite or not, (though I have witnessed the same symptom with another patient at the great hospital at Vienna,) and rather suspect that she had formerly been told, that people bit by mad dogs had an inclination also to bite. The terrible thirst she suffered forced her, from time to time, to dip her finger in the water-gruel, and moisten her tongue, which, as the nurse asserted, did, however, not increase the paroxysms of her anxieties. She now likewise felt such an aversion from the swallowing of the pills, that they must be put in her hand, which she laid on her back, and then, with her eyes shut, conveyed them to her mouth. Till two o'clock in the morning, the paroxysms of anxiety, from time to time, subsided. After that time, however, they continued without intermission till four o'clock, when the patient of a sudden became calm, dropped down, and expired. At three o'clock she had sent for me, with the request, as she was no more able to bear her oppression and anxiety, to bleed her to death; which, as well may be thought, was not complied with. Instead of this, I prescribed pills, each of which contained one grain of ipecacuanha, and one grain of opium. On their arrival the patient was no more.

Bremen, 6th December 1814.

IV.

Brief Hints, relative to the Improvement of the Pathology and Treatment of those Chronic Diseases usually termed Nervous.

By John Armstrong, M. D. Sunderland.

IT now seems admitted by the most experienced part of the profession, that the nervous affections, in certain acute fevers, are merely secondary; yet it is still supposed, that the nervous appearances peculiar to many chronic diseases are, for the most part, of a primary nature, and treated accordingly. In contravention, however, to this generally received opinion, experience has convinced me, that the majority of those chronic affections called nervous are inseparably connected with disordered circulation; and that their true pathology, like that of the nervous affections in idiopathic fevers, is to be sought in the derangements of some of the viscera, or their investments. Every practitioner must be aware how very vaguely we use medical language with regard to the class Neuroses; and, indeed, to speak candidly, under the specious terms hypochondriasis, dyspepsia, and the like, we not unfrequently conceal our ignorance from the public, and even often persuade ourselves that mere technical sound is scientific sense. If we are ever to arrive at anything like fixed principles in the treatment of chronic nervous affections, it strikes me, that it must be, not by limiting our views solely to the nervous system, as some celebrated writers have done, but by extending it a great deal farther, so as to make it comprehend many other important textures of the organic frame.

When called to cases reported to be nervous, my first object is to discover the local derangements upon which they depend; and, by an attentive examination, I have almost invariably been able pretty distinctly to refer them to some disease of function or structure within the head, chest, or abdomen; such as chronic venous congestion of the brain, chronic inflammation of the meninges, increased determination of blood to the head,* some disorder of the pleura, heart, pericardium, stomach, liver, or other internal parts. It is not my design here to detail the various symptoms by which these diseases may be detected, my object at present being simply to turn, in some degree, the professional attention, with respect to this department of medicine, into the forementioned path of inquiry, from which it has perhaps been too long allured by the false lights of some eminent authorities.

It is, however, my intention, at no very distant period, to publish an ample collection of facts, to illustrate the opinions above advanced,† and likewise to establish, that the depletory mode of treatment is by far the most safe and efficacious, in the majority of nervous affections. General or local blood-letting, purgatives, and blisters, used singly or together, have answered the best purposes in my hands; although, on several occasions, I have resorted to other auxiliary means, particularly to the warm bath and mercurials, *which are often very powerful in equalizing the circulation*; an effect of these remedies which has not yet been sufficiently regarded.

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* I have frequently seen each of these affections of the head combined with a chronic species of ophthalmia, and have often known it treated as a primary disease of the eyes: but as it is purely secondary of internal disorder, it never yields to local applications, and indeed can only be removed by removing the cause from which it proceeds. The above three cerebral affections are also closely concerned with the pathology of mania, to the last of which some specific allusions will be made at a future time.

† In this work, what are called nervous diseases will be arranged into three orders, the Cephalic, Thoracic, and Abdominal, conformably to the parts principally affected.

The debility which attends the commencement of idiopathic fevers was once supposed to be real; but this notion has surely been proved most fallacious by the experience of late years; and, agreeably to my observations, the debility which attends chronic nervous affections, particularly in their first stages, is most frequently not real, but only apparent, as it in general yields best to depletion, when judiciously used, according to the character of cases, and the age, habits, and constitution of patients. Diffusible stimuli ought seldom perhaps to be trusted as primary means of cure, and, unless exhibited with great care, they may often do great mischief in chronic nervous affections; firstly, by exciting, and, secondly, by depressing, the system too much: and the same remark may be extended, though with less force, to those medicines called tonic, the greater part of which might be erased from the pharmacopœia, without any prejudice to physic. At the same time, it is not intended, by these desultory hints, wholly to preclude diffusible stimuli in all nervous disorders, for sometimes they may be beneficially administered, when sufficient evacuations have been premised; or even immediately, under necessary depletion; as when, for example, a vital organ is oppressed with an evident engorement of blood, and the rest of the system in a really enervated state. It was one of the many errors of the ingenious Brown, to suppose, that depletion and stimulation were incompatible with each other; but there are cases, in which they may be successively, or combinedly, employed, not only without danger, but with manifest advantage. It is only by viewing diseases through the medium of the disordered function or structure of the most important parts of the body, that we can ultimately discover their right pathology and treatment; and if this remark be more applicable to one class of complaints than another, it is unquestionably to the Neuroses, which are generally local in their origin and progress, being seated in some of the viscera, or their appendages.

Wearmouth Walk, Sunderland,
July 1, 1815.

V.

Letter on the Treatment of Tetanus, to Dr Dickson, Physician to the Fleet, from Mr E. Grimstone, Surgeon of his Majesty's Ship Tonnant.

DEAR SIR,—I am favoured, by H. M. S. Asia, with your hints on Tetanus, and for which I am extremely obliged. It had been at one time my intention to have put the few practical facts I am in possession of into some form, but the late want of success in those severe cases, off New Orleans, has greatly disappointed me; although I still feel confident, that, had those remedies been administered under more favourable circumstances than that period admitted of, they would not have failed in every instance.

It would be presumptuous in me to attempt an explanation of the pathology of a disease, left in such obscurity and doubt by the first medical characters; I shall, therefore, without comment, content myself with stating the accompanying cases, as they occurred, wishing you at the same time to bear in mind, that two of them, in which I was successful, were the last that came under my care during my stay in India. I am aware of there being varieties of the disease; and those which have come within the reach of my observation admit of arrangement under three distinct heads, viz. the acute; and chronic, arising from local injuries, and considered as symptomatic; and a chronic form of the disease, considered as idiopathic, arising from more general causes; all most frequent in warm climates.

Of the last state of the disease, the greater number of cases I think do well; of the acute branch, depending on local injury, which mostly takes place where the wound (let it be great or small,) is in a highly painful and irritable state, I am led to believe, there is scarcely an instance of recovery; it generally proving fatal within the third day, in spite of all the efforts of medical aid. But it is the chronic state of the symptomatic disease in which I have been successful, and which, I feel satisfied, is what we so often hear of yielding. Here it may be remarked, that the two first species of the disease are in a great measure distinct as to the period of their attack, the acute making its appearance in a short time after the infliction of the wound, and, as I have observed above, when it is in a highly painful and irritable state; and the chronic seldom or ever taking place, until the inflammatory symptoms have subsided, or the wound is far advanced in the cure, and often many days after it has entirely healed. See Miller's case. Stimulants have done the most service in these cases; of this we have the most unequivocal evidence.

Dr Tracey, of Norwich in Connecticut, gives a case, in the Medical Journal for March 1807, wherein he was successful; and where Madeira wine, to use his own words, "was given with great freedom throughout the disease," with tinct. opii gtt. lxxx. every four hours. Here I regret to find the Doctor does not tell the actual quantity of wine given; nor indeed does he attach any credit to it at all, but gives it entirely to the opium combined with the active use of mercury.

Dr Coxe of Philadelphia, relates a case, in the London Medical Journal for November 1807, where he gave two pints of white wine in the twenty-four hours, and observes "that it appeared to exhilarate, and keep him free from that violent pain in the scrobiculus cordis, in general so severe in tetanic patients." He next day ordered the wine *ad libitum*, with large doses of opium, and tinct. cantharidis. The last brought on a violent strangury, which relieved the spasms, but which recurred as the strangury went off. On the 4th day, three pints of wine were given, independent of his "panada being made rich with wine." Wine enemas were also given frequently. On the 8th day, we find he felt some difficulty in swallowing his wine, and the "symptoms became aggravated." Indeed, in following this case throughout, (which was protracted, the patient dying on the 11th day,) the beneficial effects of the wine is clearly pointed out. The tincture of cantharidis was given gtts. 100 every hour, until 1000 were taken, and we find the increased action caused by the strangury, relieved the spasms considerably. This might strengthen our inference, that the disease is under the influence of reversed action.

Dr Christie relates a case, treated successfully by the surgeon of the 19th regiment, with opium, ether, and mercury, combined; and another, which proved fatal under the use of the cold bath, mercury, and opium, to the amount of 48 grains daily, although the mouth was much affected; and a third which also proved fatal, *after* 10 *days*, and where the symptoms were mitigated. The Doctor observes, "mercury, which soon affected the mouth, seemed to produce little effect on the symptoms."

Dr Hunter, physician-general of Bengal, has obligingly favoured me with a case, successfully treated by Dr Christie, with large doses of opium, and ether. The ultimate dose to which it was increased, was ʒiiss. of each every hour, or ℥iij. and ʒvj. daily; and the whole quantity of laudanum (of the best quality) given in 42 days, was 49 ounces 6½ drams. The warm-bath was used, which, in other cases, he had found to give more permanent relief than the cold affusion; though the last, he observes, "generally produces a temporary relaxation of the jaw, and the other affected parts."

A volume of evidence might be produced, to prove the pre-eminence of stimulants in the cure of the milder tetanic affections, in proof of which, I refer you to the latest publications of the day on that disease.

Although it will be differing from so respectable an authority as M. Larrey, yet I must say, I have never seen any permanent relief, either from local applications, or removal of parts. Amputation was performed in several cases which came under my knowledge during my service in India, and, in two cases of my own, while surgeon of the hospital-ship at Prince of Wales' Island, both symptomatic, and of the acute kind. The operation was performed on the first appearance of the disease, the difficulty of swallowing increased hourly, and they both died within the third day. Opium was administered in various shapes, and in large quantities, without the smallest effect.

Your valuable hints I shall not refer to at present; farther, than not being able altogether to coincide with you, that bleeding occasionally, and purgatives, might be a means of preventing the disease. However salutary bleeding might be, in the early symptoms of one species, I feel confident it would prove injurious, in that of which I mean more particularly to speak. Doctor Rush observes, "If I may be allowed to judge of the probable effects of blood-letting, from what I have seen to follow its use in tetanus, it will be better omitted."

But, without touching upon the importance you justly ascribe to a full and natural performance of the intestinal functions, in diseases in general, and to the free and healthy condition of the alvine discharges, as tending to obviate that in question, I sincerely hope your expectations of the utility of purgatives, if not for the cure, at least for the prevention of tetanus, "unless where the patient is exposed to the strongest exciting causes," may be realized; for I have always found the torpid state of the bowels, when once the disease has taken place, whether arising from the disease itself, or from the remedies employed, forms one of the most troublesome and difficult branches of the cure.

It appears to me, that the most successful method of treating this disease hitherto, has been by creating and keeping up for a certain period, a strong action in the system, by some powerful stimulus, greater than the morbid one already existing; but whatever is likely to promise success in this dreadful malady, should be encouraged and tried in every shape, and in every part of the globe. To look with confidence even on one of its branches, would be gaining a great point, and be relieving the mind of the patient, as well as the surgeon. I have known many refuse to allow amputation, where it was likely to have saved life, (in an hospital) from no other reason than the fear of "locked-jaw."

I am disposed to think, that, if the 5th day could be got over, we might promise ourselves success. As a general circumstance, I may state, that, in three cases of amputation of the lower extremities, among the wounded of the Dutch frigate Pallas, two, where the needle was used, had the disease, and died within the third day; and the other, where the arteries were taken up, principally with the tenaculum, did well, without the slightest symptom of it. It may not be unworthy of remark, that, in the tropics, the disease is not unfrequent among quadrupeds, the horse particularly; of which I have seen two cases.

Since I last saw you, I find my friend Mr Livingstone, surgeon to the factory in China, has published Wattel's case, which I communicated to him by letter. I have therefore sent it to you, in the same contracted shape in which he had it. It would be perhaps better they should go together, as more likely to be noticed by those who are more capable, and have better opportunities of judging of the disease itself, and of giving a more general trial to those particular remedies, which I in a great measure fell upon by accident. I am, my Dear Sir, &c.

Bermuda, April 2, 1815.

CASE.

June 3, 1811.—GEORGE PYBUS, seaman, belonging to H. M. ship Phaeton, of a full habit of body, healthy, and about 27 years of age, has been rather unwell for two days past, and complained of having had a fit last night. His pulse and skin were natural. At the time he was speaking he was seized with violent spasms in the chest, back, and loins; his jaw set; face became much bloated; profuse perspirations; pulse small and weak.

℞ Aquæ Menth. pip. ℥iss.
Confectionis aromat. ℈ss.
Tincturæ opii gtt. xx. fiat haust. statim sumendus.

Venæ sectio in brachio ad ℥x.

1 P. M.—Had another general spasm, with full hard pulse; profuse sweats; after which the jaw remained contracted, which removed the doubt I had of its being a tetanic affection; and, upon further inquiry, I found that, about a fortnight ago, he had an extensive wound just above the external malleolus of the left leg, by striking it against a gun, and which was now nearly healed; slight difficulty in swallowing.

℞ Opii gr. xx. fiant pilulæ decem. Capiat unam secundis horis, cum ℥iss. spiritus vini Gallici.

Vespere.—Had no return of the spasm; pulse full and hard; deglutition more difficult; one stool.

℞ Spirit. vini Gall. opt. ℥vj.
Tinct. opii ℥ss. fiat mist. Capiatur ℥i. tertia quaque hora.

4th.—Has not had a return of the spasms since last evening, but feels them coming on when attempting to rise in bed; jaw nearly closed; complains of great dizziness; had very little sleep; expresses great anxiety to have a passage in his bowels; speaks imperfectly. Pulse continues full.

Repetantur mist. ut heri, et spt. vin. Gal. ℥xviij. indies.

10 A. M. Ol. ricini ʒx. statim sumendæ.

I continued to give his spirits, about ℥ij. every two hours, but left off the mixture to allow the purgative to operate.

Vespere.—Had no stool, or return of the spasms; otherwise the same.

Capiat bolum calomelanos gr. v. cum solut. sulph. magnes, ℥iij. statim.

10 P. M.—Had a very bilious copious stool; belly easy; jaw closed. The mixture and spirits were immediately continued as before.

5th.—No spasm; head free from pain; slept very little; jaw closed; swallows with great difficulty.

Continuent. ut antea.

Vespere.—No spasm; very little effect from the medicine; pulse 70, and full; skin natural; perspires freely.

Repetat. mist. 4ta quaque hora. Capiat spiritus vini Gall. ℥iij. tert. horis.

6th.—Not so well this morning; no sleep till towards daylight; had two slight spasms; jaw completely closed; no stool; otherwise the same.

Cont. mistura ut heri, with as much of the spirits as could be got down.

Vespere.—Very uneasy all day; had several severe spasms, extending more towards the breast and back than before, drawing the head back with considerable force, and appearing extremely distressing. Pulse full, hard, and about 78.

Continued the spirits as before; Repet. mist. ut antea, ℥i. secundis horis. Capiat. primo cras mane bol. calom. gr. v. cum olei ricini ʒx.

7th.—Only one slight spasm during the night. At 3 A. M. left off the mixture, and gave the purgative.—12 M. Had one large and costive stool. I waited an hour, but the spasms coming on, I continued his medicine and spirits.

Vespere.—Much the same; no spasm.

Cont. ut antea.

8th.—Very little sleep; one slight spasm; jaw close; one stool since; wound in the leg healed. Pulse 70, full and hard.

Cont. mist. et cet. ut antea.

9th.—No spasm; slight stupor.

Cont. ut antea.

10th.—Had a little sleep; several slight spasms about the chest; can open the jaw rather more than any time these last four days; no stool. Pulse as before. I continued the mixture, and gave extract opii gr. i. with every dose, and got the spirits down as often as possible, for he now began to dislike it, saying it "hurt his head."

11th.—Slept better than before; feels generally easier; had several slight spasms; jaw yet more relaxed; pulse more natural; perspires very freely after each succeeding twitch; no stool since the last cathartic. I continued the brandy as usual.

Habeat statim solut. sulph. magnes. ℥iv. Omitted the mixture.

12 M.—No stool. Gave a gr. v. calomel bolus; but the spasms returned with so much violence, I was obliged to have recourse to the pill and mixture immediately. At this time the jaw is not so much relaxed as in the morning.

℞ Sodæ sulphatis ʒvj.
Infus. sennæ simp. ℥iv.
Ol. olivar. ℥iij. ft. enem. Statim injiciend. et *repet. pro* re nata.

12th.—Had one copious stool; two spasms during the night, slight, and very short.

Cont. med. ut antea.

13th.—Is generally better; two stools; jaw considerably relaxed; pulse and skin natural; the brandy and opium appear now to affect him very much; sits erect without pain or dread.

Repet. mist. ut antea, ℥i. 4tis. horis.

Omitted the pill, and gave the spirits as occasion might require.

14th, 15th.—Generally much better; can make some use of the jaw. Gave the mixture every six hours, and lessened the quantity of the spirits to ℥vij. in die.

17th and 18th.—Better. I now gave only a common wine-glass of the spirits as he appeared to want it, and the mixture as on the 15th.

19th.—Had one or two slight twitches, but is recovering fast; complains of weakness; belly open; eat freely.

℞ Extr. opii gr. xx. fiant pil. numero decem. Sumat i.
sexta quaq. hora.

22d.—Appears and feels convalescent; he now took the ship's common allowance of spirits, (℔ ss. indies) and

℞ Decoct. cinchonæ ℥vi.
Ess. Menth. pip. gtt. x.
Tinct. opii gtt. lx. ft. mist. sumat ℥i. pro re nata.

He from this period mended daily, and returned to duty July 16th. He took his opium in diminished doses up to the 10th, but now sleeps well without it.

Case.

Robert Millar, about 20 years of age, of a slender make, belonging to his Majesty's ship Phaeton, had received two musket-shot wounds on the 30th August 1811; one in the upper, and the other in the lower left-arm. They are now perfectly healed, and have been so for some days. The flexor muscles rather contracted. September 26, 1811.—He is in tolerable health; says, for two nights past, he has had sudden twitches, and his mouth has become as suddenly contracted, and that he was unable to speak; pulse small, and rather hard.

℞ Extr. opii gr. ii. statim, et repetatur gr. i. tertia quaque hora.

Vespere.—No alteration; no spasm. Cont. pil.

27th.—Had many spasms during the night, and his jaw remains more contracted; pulse and skin nearly natural; belly open.

Repetatur extr. opii gr. i. secundis horis, cum spiritus vini Gallici ℥iss. singul. pil.

Vespere.—Spasms frequent, but not violent; added gr. ss. more of the extract to each pill, and continued as before.

28th.—Had many spasms during the night, extending to the back and breast; jaw, in the intervals, opens about half an inch; bowels open; pulse and skin natural.—Cont. ut heri.

4, P. M.—Had a severe and distressing spasm; jaw remains close; otherwise the same.

℞ Pil opii gr. ii. cum spts. vini Gall. ℥iss. secunda quaq. hora sumend.

29th.—Remains in the same state; spasms very frequent, but not so violent as the one yesterday, or when the interval is greater.

Cont. med. et cetera.

30th.—Very little alteration; slept a little; two stools; pulse and skin nearly natural.—Cont. pil. ut antea. cum spts. vini Gall. ℔ ii. indies.

October 1.—No spasms until two o'clock this morning; they were less severe than before; a considerable degree of stupor from the spirits; otherwise the same.

℞ Ext. colocynth. comp. ʒss. Solve in spts. vini Gall. ℔. ii. et capiat. sol. cum pil. ut antea.

2d.—Spasms rather frequent during the night, extending to the shoulders; two stools; jaw the same.

Repetantur medicam. ut antea.

3d.—Had four rather severe spasms during the night; the spirits appear to have greater effect than before. Cont. ut antea.

4th.—Spasms more general last night than before, but he slept considerably, and begins to dislike the effect of the spirits; belly open; pulse and skin natural, except what might be expected from the medicines.

Repet. pil. ut antea, cum spts. vini G. ℔ iss. indies.

5th and 6th.—The jaw remained closed until this morning, when he opens it more than half an inch; had during these days several spasms, but they were weaker; belly open; health apparently good.

Cont. pil. et spts. ut 4to die præscript.

7th.—Had four slight spasms in the night; but the jaw less confined, and feels generally better.

Repetantur med. ut heri.

8th and 9th.—Spasms still frequent, but very slight; feels better; belly open; reduced his spirits to one pint daily, and his pil. to gr. iss. every second hour.

From this time, the attacks were less frequent, and got daily weaker. On the 10th I reduced the extract to gr. i. every second hour, the jaw opening nearly to its whole extent; on the 12th I gave the pill every four hours, and on the 16th every six hours, and reduced the spirits to half a pint daily; on the 21st he was free from complaint.

Pybus's, I think, was a well marked case of that species of the disease; and this (Millar's) may be called a mild one; but I cannot think otherwise, than that it would have terminated fatally, had not the spirits, or some other powerful stimulus, been combined with the opium; at least, I have seen one equally mild end in death, under the use of the cold bath, and opium in very large doses. I have always looked upon the stupor, produced by the spirits, or sleep from the opium, as very favourable circumstances, neither seeming to have any determined effect, until the disease is on the decline.

CASE.

Mr CHARLES WATTELL, clerk of His Majesty's brig Diana, aged about 30 years, of a spare habit, was received into H. M. hospital-ship, Wilhelmina, on the 20th May 1809, for the cure of ound, caused by a pistol ball passing through the palm of e right hand. The integuments were much lacerated, and the middle metacarpal bone considerably shattered.

Upon first viewing the case, I advised amputation above the wrist, giving him my reasons for so doing, founded on the great probability of a locked-jaw taking place, particularly in this climate, but he would not consent. I then dissected out both parts of the metacarpal bone with the finger, and brought the integuments together, on both sides of the hand, by stitches; applied soft dressings, and gave extract. opii gr. ij.

22.—Considerable heat; costive; hand and arm very painful.

℞ Mist. nitros. ℥vj.
Tinct. opii gtt. xxx. ft. M. capiat. cochlear. duo tert. qq. hor.
Vesp. Extract. opii gr. ii. et cras mane sol. Sal. Glaub. ℥iii. which had the desired effect.

24th.—Much less pain; pulse moderate; skin cool, and the bowels open. Dressed the wound, which looked and discharged well.

℞ Pulv. cinchonæ ℥ss.
Spt. vini Gall. ℥iss.
Aquæ puræ ℥viss. ft. mist.
Sumat ℥i. tert. qq. hor. *Vesp.* Ext. opii gr. ii.

He continued to do well until the 8th of June, when the wound was nearly healed, and I observed signs of tetanus, such as difficulty of swallowing, turning the head, alteration of speech, and the jaw slightly contracted. On inquiry, I found he had general spasms for the two last days past, particularly violent about the neck and back, which he had neglected to tell me of before. I gave him tinct. opii gtt. xx. statim, and repeated ten more every hour. Again pressed him to submit to amputation, but he still refused; consequently I applied a pledget, dipped in equal parts of tinct. opii et spts. terebinth. to the wound.

June 9th.—The jaw completely set, so as not to admit of fluids but with difficulty; spasms general and violent; no stool since the 7th instant.

℞ Tinct. opii ʒi.
Aquæ puræ ℥viij. Capiat. ℥i. omni hora. Put him into the cold bath.

10th.—No alteration. Continued the mixture as before, with tinct. opii ʒiss. et tinct. digitalis ʒss.

℞ Calomel. ppt. gr. viii. statim sumend. *Vesp.* Solut. sal Glaub. ℥iiss.

11th, 12th, and 13th. Spasms extremely distressing; costive. Ol. ricini ʒx. statim.

℞ Tinct. opii ʒij.
Aquæ puræ ℥viij. M. Capt. ut antea.

Vesp. Bowels open. Cont. mist.

14th.—I put ʒiiss. tinct. opii to the mixture, and on the *17th* ʒiii. during which time his sufferings were very great. I had kept the bowels open. Wound healed. This morning the spasms were frequent, but certainly not so violent. I now, from the length of time, and the diminished state of the symptoms, began to entertain some hope. Continued the medicine, as above, till the 24th, without any material alteration. Pulse generally quick, and inclined to fulness. Previous to my visiting him this evening, my assistant told me that the spasms had entirely gone off. On my going to him, I found the jaw much relaxed, and he said he was free from pain, but to all appearance much intoxicated; and, on further inquiry, I found he had been very much so in the early part of the day. I directed the mixture to be continued as usual.

25th.—The spasms were general, violent, and the jaw contracted more than ever; had one stool; pulse not so full; profuse perspiration; and complained of languor. I continued the mixture, as above, every hour, and gave Extract opii gr. i. every two hours, and rather better than an ounce of good spirits *every hour.* I continued this until the 28th, when he was evidently better. Cont. med.

July 1st.—Still better; could take slightly of solids, and use the jaw without bringing on the spasm.

℞ Ol. ricini ʒx. statim. Cont. mist.; and gave gr. *ii. of the* extract of opium every second hour.

From this day he gradually recovered. I made no alteration in his medicines, excepting omitting a part of the spirits, till the 10th, when he could open the mouth, and make use of the jaw as well as ever. The debility was great, and now and then a slight spasm takes place, from the right shoulder to the temple of the same side; but it diminishes daily.

During the progress of the disease, I observed the quantity of opium given neither affected the head nor procured sleep. He generally complained of want of rest. The tincture was made by myself, and very good. I took the idea of preferring it to the extract, from the writings of Professor Mursinna of Berlin. I also observed, in the use of the cold-bath, that, although the interval was somewhat lengthened, the spasm followed with greater force; and I am much in doubt whether the digitalis had not a similar effect.

Whether may we attribute the success of the case to the stimulus kept up by the spirits, or to the antispasmodic effects of the opium, I am at a loss to know. I should rather conclude it was the combined action of both; and although deserving of further trial, it would be dangerous to depend on the spirits alone.

I must further observe, that my patient was one who indulged freely in the use of spirits when in health; consequently, we might expect its stimulating qualities would have less effect on him than on one who had not done so.

VI.

Observations on the treatment of Gunshot-Wounds. By GEORGE POWER, Surgeon to the Forces.

ACCUSTOMED for many years to exercise my professional duties on the field of battle in different parts of the world, and subsequently in general hospitals on the Peninsula, I cannot refrain, on the present occasion, from offering a few practical remarks on the nature and cure of gunshot-wounds, hastily collected from manuscript papers, intended for publication on some future day; with a view to the improvement of the junior branches of the profession.

To those but slightly acquainted with the history of surgery, and the imperfect state of its doctrines, it must occasion not a little surprise, to find that a disease of almost daily and universal occurrence, and which has been noticed by numerous writers since the invention of gunpowder, should at the present day be involved in doubt and obscurity, and that the widest differences of opinion should exist, both with regard to its nature and mode of treatment. I allude to that disease denominated gangrene, which is the frequent concomitant of gunshot-wounds.

The keeping in view the existence of a morbid vascular action in wounded parts, may be productive of advantage in various respects; but more especially with regard to the use of heating and intoxicating medicines; as spices, wine, opium, and the like, which are too often at the present day carried to a dangerous length, in endeavours to resist the progress of gangrene: and, as "it is far more within the power of the practitioner to reduce increased excitement and to check excessive action, than, by rousing new energy, to suspend the tendency to sphacelus and gangrene," * or, in other words, to decomposition and death. I have great reason to be persuaded that many patients have fallen, and will long continue to fall victims in *every* part of the world which I have visited, to the effects of bark, wine, opium, camphor, brandy, &c. &c. administered when a dark coloured spot, appearing on the surface of a wound, seems to authorize the use of these highly stimulant remedies.

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* *Vide* Dr Thomson's Lectures on Inflammation.

Gunshot wounds in general, either fracture the bones, or lacerate or divide tendons, ligaments and aponeuroses; and whether it be that the health or life of the muscles depend upon the healthy state of these bodies, or that the ball in its passage disorganises or decomposes the structure of the muscle through which it penetrates before it reaches the bones, &c. &c. certain it is, that the disease of these bodies seems to induce a tendency to decay in the contiguous muscular parts. And whoever seeks the cause of the appearances, which they assume, in the effects of air, contagion, &c. &c. and expects to produce a cure by the use of topical applications only, in gunshot wounds, will often be deceived in the one and disappointed in the other, notwithstanding that many of these *media* have a powerful influence in promoting and preventing putrefaction.

When a gunshot wound fractures bones, lacerates tendons, &c. &c. until these diseased bodies are either restored or removed, the wound will seldom heal; but will generally preserve a tendency to gangrene; the repeated approaches of which are always indicated by the elevation of its edges, and a hardness, heat, and redness of the parts surrounding the wound. In those cases, I have found the antiphlogistic mode of treatment the most successful; and under these impressions, I do *not hesitate* to recommend, where the appearances above stated occur, the antiphlogistic treatment, as being the most effectual in preventing the approach of gangrene and sphacelus, these not being synonymous terms, as is generally asserted, but distinct stages of disease, the one the commencement, the other the conclusion of mortification; the symptoms of inflammation in the first case not disappearing before gangrene is established, but undergoing an imperceptible change from one to the other; the latter being the complete death or mortification of the part in which it occurs.

With these views I have employed large and repeated bleedings, and always with peculiar good effect. Next to these, purgatives, abstemious diet, particularly as to animal food, cleanliness, and lastly, abstraction of heat from the part, by cold applications, so modified as to excite in the patient pleasing sensations.

By this plan of treatment, if the injury sustained is not very extensive, many serious wounds of the head, trunk, and extremities, will be conducted to convalescence, which there is reason to believe under opposite treatment would end in gangrene and sphacelus.

The foregoing observations apply to the practice to be employed in the cure of those stages of disease which are the consequences of gunshot wounds only; hospital gangrene and mortification, proceeding from constitutional and other causes, being of a nature totally different, and requiring other treatment.

Ophthalmia Depot, Maker Heights, June 27th 1815.

VII.

Letter from Mr Carmichael *in reply to the Review of his Essay, &c.*

To the Editors of the Edinburgh Medical and Surgical Journal.

Gentlemen,—I do not complain that my lot should differ from that of others. It is very provoking to be sure, but it is the common course of things, that, when a man has made some necessary improvements, and ascertained some useful facts, and candidly communicated all his information to the public,—when he expects at least the tacit accordance of every thinking individual with the truths he has established, and the thanks of society for the benefit redounding from them,—it is very provoking, even though it is the common course of things, to flounder into the disappointments prepared to receive him; to find his labours but begun which he thought were completed; and that he has half a score of reviewers to contend with, who have read his work, not with a view of discovering whether it contains any thing of value, but whether it can be sapped and undermined; and, though constructed of the very adamant of truth, elaborately bored through, and blown to perdition.

Yet surely, Gentlemen, you will allow that it is the first virtue of a reviewer to cultivate a love of truth. He of all men ought to be the most unprejudiced by interest, unbiassed by indolence, and ungoverned by stubborn, ignorant, jog-trotting pedantry. It is his duty to be the first to hail with applause a beneficial discovery, and stamp with all the credit he possesses those novel truths, which the generality of persons are disposed to reject, merely because they are novelties. As to himself, truth should be the first and last of his virtues; as to others the *first* and last of his objects; his only sentiment, his only research.

I wish my work was of that nature that it could have fallen under the strictures of the true Edinburgh Reviewers; I should not then have so much occasion to regret that the *intellectual city* (that well-deserved appellation, and to whose propriety they so largely contribute) should produce reviewers not altogether possessing the qualities to which I have alluded, and without which the trade of criticism is more pernicious than serviceable to society; but whose captiousness and abuses bring with them their own remedy, and, like locusts or musquitos, are only mischievous or troublesome while endured, but may be crushed with a finger, or blown away with a breath.

These observations you may apply, Gentlemen, if you please, to the person you sometimes employ to write reviews for your Journal, and into whose hands my essay on the diseases which have been confounded with syphilis fortunately fell:—Fortunately, I repeat, as his strictures cannot but tend to establish the value of my facts and the truth of my principles. You yourselves shall be the judges between us; and if, in your next number, you do not acknowledge that in the whole I am right and my adversary wrong, you have neither the candour nor good sense I, in common with the world, am inclined to allow you.

Let me however be candid myself. I admitted on a former occasion, and I again repeat, that that gentleman's critique on the first part of my essay, "though not much calculated to feed my vanity, manifested great clearness of comprehension, candour and ability," and why he should so unnecessarily abandon the high character he ought always to have been proud to maintain, I cannot conceive, unless indeed he might have been angry at a smile that escaped me in the postscript to my work; as it often happens, that persons most in the habit of raising a laugh against others, can least endure even a smile at themselves. Be that as it may, I shall proceed to investigate his entire critique, except those points I considered in the postscript alluded to, that it may not be said, that, in confining myself in a great measure to his objections to the second part of my work, I omitted to notice any cavil to be found throughout the whole of his strictures.

His first assault is comprised in an observation, that "it would appear from the advertisement prefixed to the work, that this investigation was begun by *assuming* the principle which is now maintained as the result of it."

But surely the reviewer must be a very indifferent metaphysician, if he is not aware that, in every course of experiments in search after new facts, it is almost a matter of necessity to presume the possible existence of that, whose positive existence or non-existence the experiments are calculated afterwards to ascertain and put beyond doubt. The mind must have some preconception of the discoveries it is seeking after, or none can be possibly made except by mere accident.

The reviewer next expresses his surprise "on reading that part of the treatise which relates to the constitutional symptoms originating from primary ulcers on the genitals which are not chancrous, and of which the author has described three species, to find that these secondary symptoms are also limited to one species of eruption, and that of a papular character," and he triumphantly asks "what are we to say of those numerous cases, which we thoroughly believe we have seen, of pustular and tubercular eruptions, terminating in ulceration, which have distinctly followed primary affections of the genitals, and have yielded only to a mercurial course? We do not appeal to general experience, but to the few best informed practitioners; because we are certain, that multitudes of these eruptions are every day hastily pronounced to be syphilitic, which are not so, but to admit the statements of Mr Carmichael to the full extent, seems to be little less than giving up the evidence of our senses." But it appears that the reviewer satisfied himself most abundantly in the second part of my work that I described more than *one* species of eruption; as he proclaims in his review of that part, that in my four engravings he discovered no less than *six* forms of eruption (p. 377.) Now I have no reason to complain of Mr West who made the drawings, nor of Mr Stewart who engraved them; and every person who has had an opportunity of witnessing the eruptions, will acknowledge the accuracy of the artists in question; yet I solemnly aver these gentlemen painted and engraved for my work four forms of eruption and no more, viz. the scaly, the papular, the pustular, and the tubercular, which comprehends the sum total of all I have ever observed; and how my sharp sighted critic could discern a number amounting to one-third of the whole more than really existed I am at a loss to conjecture. If he could so multiply the species of venereal eruption in four little engravings, consisting at the utmost of sixteen figures, where would his imagination wander in a huge Lock Hospital of three hundred patients and upwards? Instead of four, which he would probably find there, he would multiply the shades and varieties of those strong characteristic appearances to forty species at least, and then the inference must be "that Mr Carmichael had not seen, or not marked these multiform appearances, and was therefore *generalizing* from a *very limited* collection of facts." I thought I had overburthened my work with facts; but before long he shall have the opportunity of satisfying himself; and I trust that he will be candid enough to acknowledge, that my collection is not so very limited, nor my generalization so very unfounded, as he here seems determined to think.

But he has fallen into another oversight with respect to my plates. "We have little doubt," he observes, "that eruptions, not distinguishable from each other in external character, are sometimes syphilitic and sometimes not, and the *first syphilitic* eruption figured by Mr Carmichael is probably in this predicament. It appears to be the same eruption which has been recently figured in the Medico-Chirurgical Transactions of London (Vol. V. Art. 14.) as one of those equivocal diseases which is often curable without the assistance of mercury."

If he had compared my description with that of Dr Bateman, the author of the article he alludes to, he would have found that these eruptions were totally different. My plate represents an eruption, of which Dr Willan has also given a description and a plate, and distinguished it by the name of the syphilitic psoriasis. The description I annexed to the figure would have informed the reviewer that the spots in question were possessed of that *scaliness from the commencement*, which is the peculiar characteristic of the syphilitic eruption. But he would have learned from Dr Bateman, that the eruption in *his* engraving "consisted of a number of small circular spots, from one to two lines in diameter, slightly elevated above the surface of the cuticle, but flattened on their own superficies. They were at first of a dusky rose colour, *smooth and shining;* but subsequently they became somewhat darker, or of a more purple and chocolate hue, and the surfaces of some of them *at length* exhibited a slight tendency to desquamation in the centre."

No person of acuteness who read this description after a perusal of my essay, but would instantly decide that this eruption was not syphilitic, but one which would disappear, to use the words of Dr Bateman, "with its concomitant evils, without the use of mercury in any shape." Dr Bateman saw eight or ten cases of this disease, which he calls the Tubercular eruption; and states, that it is unnoticed by medical writers, and was not included in the enumeration of Willan. This very eruption I have seen, and accurately described in my work as tubercular. It will be found in the case of Arthur Bassett, (LXXXIV.); and if the reviewer takes the trouble of perusing page 208, he will probably be surprised to find in how many points the case alluded to corresponds with that of Dr Bateman. His erroneous remarks on this occasion would scarcely have dropt from the reviewer, if he had himself seen instances of these two eruptions. Engravings are in some, and perhaps a great degree, useful; but he that abandons his clinical investigations to study diseases in plates, will not make, I apprehend, a very enviable proficiency in his knowledge of those or any other diseases. For example, a painter could never express with his pencil that an eruption was scaly from the *commencement*; yet without this circumstance it could never be known from inspection that the drawing represented a syphilitic eruption curable by mercury, and mercury alone; and therefore the reviewer would have avoided the oversight in question if he had compared the descriptions, as well as the plates.

It will be necessary, however, to follow a little farther his objections on the subject. He observes, that, "satisfied as we are that numerous local and constitutional affections are perpetually pronounced to be syphilitic, and treated with mercury, which are not venereal, and are even aggravated by that medicine, yet we cannot look upon the first plate of Mr Carmichael, on which two eruptions only are delineated, and assent to the statement, that no other eruptions than these require the full impregnation of mercury for their cure, or are the result of the true primary symptoms of syphilis; nor do the other three or four cutaneous appearances, which he has delineated, comprise a twentieth part of the various affections of the skin which are seen in the cachectic states which follow primary ulcers in some part of the body, or arise where no such ulcers have been seen." To this objection I shall only observe, that the syphilitic eruptions delineated in Plate I. "yield with certainty and celerity to the action of mercury;" that my cases demonstrate that this medicine has not the same certain effects on any other form of venereal eruption; that sometimes those different eruptions, distinguishable from syphilis, yield to the remedy, yet they as frequently return; but often they resist its influence altogether, and, with the constitutional ulcers which they form, they spread so rapidly under its use, as to demonstrate it on many occasions to be actually injurious. If, then, mercury is thus dubious or detrimental in its effects on eruptions of certain characters, while it is always beneficial on those of another description, surely it is useful to point out those distinctions of character, and to put practitioners on their guard against the full exhibition of mercury for those eruptions over which it exerts so doubtful or pernicious an influence. Whether any eruptions but the scaly require, under any circumstances, the full exhibition of mercury, is the great point at issue; and which, I flatter myself, the host of evidence I have brought forward, goes a great way to decide in the negative.

As to my plates not comprising a twentieth part of the affections of the skin, &c. &c. it would be a silly affectation of modesty to submit to the charge. They represent all that I considered useful or necessary, and all that I intended they should represent, viz. the most common examples of the four different species under which all venereal eruptions may be comprised,—the Scaly, the Papular, the Pustular, and Tubercular. But if all the varieties could be collected and exhibited in plates, they would greatly and unnecessarily enhance the price of the work. I have by me drawings of the most frequent and remarkable, which perhaps at some future day shall be laid before the public. In the meantime, the figures I have given are amply sufficient to convey an adequate idea of the eruptions alluded to, and afford every facility of comparing and distinguishing them. With respect to this objection, however, I shall beg leave, while on the subject, to cite the following passage from my essay, which seems to have escaped the observation of the reviewer.

" It is probable that I have not noticed all the varieties of venereal appearances which occur in this country, but I believe that I have drawn a strong, well-defined, and faithful outline of the real and substantial distinctions which are marked by nature, and no more could rationally be expected. The shades of difference which occur in diseases are endless, but with respect to those which are more immediately our object, the judgment of the practitioner may readily fill up the outline, by placing such varieties as may happen to fall in his way, in their appropriate places," &c. &c. But our reviewer proceeds:—

" We believe, too, that when he ascribes that opinion to *mistake*, which maintains, that the excessive use, or the peculiar operation of mercury, does produce *eruptions*, *ulcers on the skin and throat*, *and nodes on the bones*, which are cured by the omission of the mercury, and the substitution of other means, he applies the word *mistake* to the sober inferences of some of the most experienced practitioners of this country, whose opportunities, both in public and private practice, for a long series of years, have been probably superior to his own. Indeed we conceive that his experience must have been very limited whose personal observation has not satisfied him of this fact." In reply to this, I beg to observe, that I have not, nor do I believe that any other person has witnessed, ulcers on the skin and throat, and nodes on the bones, from the exhibition of the most extensive courses of mercury in any other than venereal diseases, nor even an eruption, except the well-known mercurial eczema. Whe therefore, such symptoms occur, we may reasonably conclude that they are the product of the animal, and not of the mineral poison.

" It seems manifest," says our critic, " from this chapter, (the 4th, which treats of the phagedænic ulcer) that the experience of our author, which he deems sufficient to develope the nature of those obscure maladies, which have employed the lifetime of many an able observer, is very brief, and of recent date. We find most of his cases of no earlier date than 1812, two years only before the commission of this treatise to the press ; and so late as the summer of that year, we find him first discovering that these phagedænic ulcers, and certain other corresponding affections of the throat and limbs, were not relieved by mercury, but were aggravated during its use ; and then first adopting *a new view of the subjeet,* which is, however, perfectly familiar to experienced surgeons, namely, the necessity of abandoning the administration of mercury, and of resorting to sarsaparilla, good air, and other means of improving the general health under such circumstances." To these objections the reply is so obvious, that I cannot conceive how my commentator could seriously urge them ; for it is evident, that as long as the minds of the profession are pre-occupied with the impression that those phagedænic ulcers were syphilitic, no matter the length or extent of their experience, mercury would of course be employed, and the true nature of the disease remain undiscovered. But, if there was no other consideration, the number of cases I detailed of these ulcers which were cured without mercury, ought to have satisfied him of the justice of my principles. But so far is my critic from infallibility, that he is mistaken in his assertion that it was not till the summer of 1812 that I found mercury injurious to phagedænic ulcers, for he might have seen in Case XLVII. which occurred in 1810, that I postponed the use of mercury until the inflammation was removed, and employed warm fomentations and poultices, &c. But, in my then ignorance of the true mode of treatment, I afterwards resorted to the still common practice; and when the irritation was removed, and the ulcer was healing, I again had recourse to mercury, the ill effects of which induced me to abandon, from that period, full courses of that medicine in all other similar cases. The result of this novel and most successful treatment, the critic does not think proper to notice; and so far from giving me credit for the short period in which I had brought my practice to such perfection, he makes it matter of abuse that I should accomplish, with such brief and recent experience, " what has employed the lifetime of many an able observer."

After this instance of candour, he proceeds to urge his opinion, in opposition to mine, that these phagedænic and sloughing ulcers are "truly syphilitic in the outset, but that the syphilitic virus had been *eradicated by the specific*, * and these phagedænic dispositions had ensued, either as the sequelæ of lues venerea, or the effects of mercury in a bad habit of body, to borrow the words of an author very well informed upon this subject. For we are disposed to believe, with this able and experienced writer, (Mr John Pearson, in his admirable work on the effects of various articles of the Materia Medica in Lues Venerea,) that the venereal virus, when introduced into the system, often gives rise to morbid appearances, which do not in any proper sense partake of the nature of the remote cause."

Now, as I have given several instances of primary ulcers, which were phagedænic or sloughing, and as I stated that I had an opportunity of observing from the outset, that they had not any of the characters of chancre, I cannot well conjecture how, in contradiction of those facts, the reviewer can theorize these ulcers into sequelæ of syphilis, which in plain English, I believe, means the sequel or consequences of lues venerea, or, as the vulgar call it, *the dregs* of the disorder. And in opposition to the fact, that many of these patients were cured without mercury, (see particularly Cases LI. LII. LVII. and LXI.) still less can I comprehend, why the critic adopts the still more inconsistent hypothesis that these ulcers were the effects of mercury on a bad habit of body.

We next find the following passage, which I am compelled to give at full length: "With respect to the eruptive disease which the author asserts to be peculiar to this phagedænic *ulcer*, in common with the sloughing ulcer, which he next describes, it appears to us, that nothing approaching to proof of its identity, or of its exclusive connection with these ulcerations, is here stated He has figured a pustular and scabby eruption, the form of which is by no means definite; and in two or three cases, in which an eruption is said to have followed the phagedænic ulcer, any sort of crust, scab, or scabby ulcer, appears to be deemed a specimen of the same eruption. A circular crust the size of a shilling, situate on his forehead, with a surrounding red pimply eruption, (p. 175); pustules scattered over her body, but more numerous on her back, than on any other part, which were of a large size, and as distinct as those which occur in a case of mild small-pox, (p. 177); three spots on his face formed by large brown conical crusts, one of which, in particular, projected like a horn from his forehead, (p. 179); a large spot covered with a crust on his right elbow, which he states to have been there a fortnight, (p. 147);—these are all given as specimens of the same identical eruption, the secondary product of the two ulcers just mentioned! In one case, indeed, (p. 179,) an eruption is figured as a specimen of this secondary symptom, though it is only conjectured to have been preceded by one of those ulcers, from its alleged similarity to another case, no primary symptom having been in reality seen! In almost all other cases, related by the author, a sore throat, without any cutaneous eruption, was the principal secondary symptom of these ulcers."

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* If an author might presume to offer advice to a reviewer, I would strongly recommend our critic to peruse a page or two of Dr Adams on the subject of this very phraseology, which, if it were less absurd and unphilosophic than Adams conceives, a gentleman in his vocation ought surely, before this time of day, to have abandoned. (See Adams on Morbid Poison, 2d edit. from p. 141 to 151.)

I do not know what description of evidence the commentator requires in proof of the connexion which I have found to exist between the phagedænic primary ulcers, and a pustular eruption, which quickly forms ulcers covered by crusts. The proofs I have offered are these: In four cases very particularly detailed, I found existing, at the same time, the primary and the secondary symptoms, the phagedænic ulcer, and the pustular scabby eruption, and I should suppose these were sufficient for any reasonable inquirer. But it may be satisfactory to my sceptical critic to know, that, since the publication of my work, I have met with several similar proofs of this connection, and not one in subversion of a fact so important in the investigation of venereal diseases. Indeed I am overwhelmed with the multiplicity of cases which have occurred since the appearance of my work. I have transmitted a few of them to my bookseller in London, for insertion in one of the medical journals of that city, and they cannot but have an effect on incredulity and prejudice, however inveterate.

I beg of my reader to observe, that, previous to my adverting to the several isolated symptoms, so industriously collected in the above quotation, I had, in p. 125 of my work, given a general and accurate description of all the symptoms of the constitutional malady arising from the phagedænic and sloughing ulcers. Any reader of intelligence, on finding an unconnected symptom, would naturally return to the general description of all the symptoms of the disease; nor would any reader of intelligence endure to be told every moment,—This is one of the symptoms to be found in my general description, p. 125. That general description, together with the excellent plates of the eruption, would be amply sufficient to satisfy any inquirer, if he were not a critic by trade. Again, my own strictness in giving every circumstance to my reader precisely as it appeared to myself, has been another source of dissatisfaction to my reviewer. The pustular eruption, Plate IV. fig. 2. which was so strongly marked, that I was induced to give a drawing of it, as affording a good example of this species of venereal eruption, is objected to, because, it is said, I *conjectured* that it was preceded by one of the ulcers in question; but it would have been too candid in my censor to have given my words, "that there was no opportunity of witnessing the primary ulcer; but, as the eruption agreed in character with that of the phagedænic ulcer, it might be considered as appertaining to that species; neither does the reviewer think proper to advert to the distinguishing characters which mark the ulcerations in the throat attendant upon the phagedænic ulcer, and which I have most accurately described. He is contented by simply calling it a "sore throat," for the purpose of imputing to me the confusion and indistinctness he himself thus creates. If he had but given my details the slightest share of his attention, it would perhaps have been more obvious to him, that the "specimen of induction" he criticises was of a "very philosophical character," and that the "inferences cannot be considered" *otherwise* than as legitimately established.

"We are rather inclined, indeed," the reviewer, in the next place observes, "to consider the tendency to ulceration, if not arrested by the specific, as inherent in all the syphilitic eruptions, both upon the authority of sound and long experienced observers, and of our own remarks." Surely in no part of my work has the reviewer found a denial of this fact; on the contrary, in p. 43, the manner in which the syphilitic eruption proceeds to ulceration is described, and I have even given in fig. 1. Plate I. a delineation of the ulcerous scab it produces.

"We do not mean to contend however," he proceeds to remark, "that these phagedænic and sloughing ulcers of the genitals do not occasionally occur, independently of the poison of syphilis, or of mercury, or of the combined operation of both; or that they are not sometimes communicated by sexual intercourse where no actual syphilis exists." After comparing this passage with one which I have quoted above, wherein he attributes, with Mr Pearson, the phagedænic ulceration "to the sequelæ of lues venerea, or the effects of mercury on a bad habit of body," I am at a loss to discover what is the real opinion of our commentator on this knotty point of pathology; but I would advise him to continue to vaccillate between the two opinions, until his mind is decided by "the long promised treatise on lues venerea" from London, and not to allow it to be influenced by any facts or doctrines which may come from a provincial city like Dublin.

The reviewer continues: "We confess that we are a little surprised to find the author setting out with this annunciation, that the task will not be very burthensome, for one species *of eruption* only remains to be discussed, all the others which I have met with having been already described and traced to their source. (p. 187.) We began to recollect the extreme variety of cutaneous eruptions, which we imagined our own observation had presented to us of equivocal character, and accompanied by nodes and pains of the limbs, by ulcerations of the throat, inflammations of the eyes, emaciation, &c." The reviewer here thinks proper not to understand the meaning I applied to the term "*species*." All the venereal eruptions I had met with were either Scaly, Papular, Pustular, or Tubercular. Those of the three first characters were already described, the last alone remained to be considered. This, from the context, was so evidently my meaning, that it could only be wilfully mistaken. Our critic, however, possibly adopted his meaning of the passage, for the purpose of contrasting it with the more prudent caution of a gentleman for whom I, in common with the public at large, have the highest respect; but Mr Pearson can derive but little pride from the adulation of an eulogist who proves, when he criticises, that he feels not what he most ought to feel, a passion for truth. The encomiastic strain thus proceeds: "We remembered the difficulties expressed by one of the most learned and able surgeons of the age, already quoted, after an attendance of thirty years on the Lock Hospital of London, and the enjoyment of extensive general practice, respecting these spurious appearances. 'I have not yet attained to that complete and satisfactory knowledge of the *cachexia syphiloidea* which would authorize me to obtrude a publication on the subject; but the experience I have already had in the treatment of that *multiform disease*," &c. &c.

Now, as I am fully of opinion, and that opinion strongly built upon facts, that those spurious appearances, as they are termed by the reviewer, are not the product of the syphilitic, but of other poisons, I ought to have been pardoned for not adopting the opinion of Mr Pearson, however in general I value it, nor the term he employs of *cachexia syphiloidea*, which is so highly admired by his eulogist, and which, I admit, was a very good appellation in the state of our knowledge when Mr Pearson first used it; but, as we were then ignorant of the subject, he was necessarily compelled to resort to the common shift, of choosing a term calculated to satisfy our ignorance when real knowledge is wanting. But I was not under that necessity. Thanks to the previous exertions of Mr Abernethy, and those profound and perspicuous remarks with which he stimulated me to the investigation in which I humbly followed him, that ignorance (if I may speak without humility, where humility would be weakness) that ignorance was dissipated, real knowledge was established, and I was enabled to designate (as in the chemical nomenclature) things by names, which signified their nature.

My reviewer next expresses his disappointment as follows: "After a considerable detail of cases, of which, indeed, this second part principally consists, we expected to find the inquiry into the probability of the spontaneous origin of some diseases which bear a resemblance to the constitutional symptoms of syphilis," (to which, the author observes, his cases naturally lead, and which he announces in capitals.) "We expected to find this inquiry, we say, occupying the author's most particular attention. We were, therefore, not a little disappointed to find, that he absolutely avoids the inquiry altogether, contenting himself with referring to Mr Abernethy, and stating briefly the general fact, that the health is in all cases previously disordered, and the chylopoetic organs especially deranged. But, with all deference to the acknowledged genius of Mr Abernethy, which is entitled to the highest respect, it does not appear to us that much light is thrown upon the pathology of diseases by an observation so vague and general as this." Notwithstanding my regret at having disappointed my reviewer in not entering further into this inquiry, I fancy my reader will think that I did quite enough in referring to Mr Abernethy's work on diseases resembling syphilis, and my own on scrofula, in which it is demonstrated, as far as a pathological inquiry will admit, that swelling and ulceration of the lymphatic glands, various cutaneous affections, but particularly the tubercular eruption terminating in ulceration, and even ulceration of the throat, all symptoms apparently venereal, arise from disorder of the digestive organs. I might have repeated what I said myself, or, with still greater advantage, what was said by Mr Abernethy on the subject; but, as I am not much disposed to write, nor the generality of practitioners to peruse what has been already promulgated, and with which my reader might be as familiar as myself, I scorned, as much as Mr Shandy, "to make use of the advantage which the fortune of the pen gave me over him, force him upon this hard service, and make him pay for fifty pages which I had no right to sell him." I thought it more honest, and indeed more advantageous to all parties concerned, to dispose only of goods which were new and unworn. These were four or five cases elucidating Mr Abernethy's views and my own, and which, *as facts*, form a very material part of my investigation of this particular subject. The investigation in question occupies half-a-dozen pages, and would have very unnecessarily multiplied themselves into a dozen times the number, if I had entered into the routine which would have pleased my reviewer.

He next makes some remarks upon the administration of nostrums, which I do not comprehend; and, as I do not believe they were intended for me, I need not trouble myself about an answer; but he adds, that "scrofula, and these syphiloid maladies, are not the same diseases, and are not curable by the same remedies. Where, then, is the philosophy or the practical benefit of imputing to them the same origin in these vague hypothetical terms?" If by syphiloid maladies, (as he is still pleased to call them) the reviewer means the spontaneous diseases just alluded to, I tell him explicitly, that so slight, if any, is the difference between them, and so strong their accordance in every particular, that scrofula and these maladies may almost be considered the same diseases, and are certainly curable by the very same remedies. Swelling and ulceration of the cervical glands are frequent symptoms of those apparently venereal but spontaneous disorders, but are usually attributed to the mercury employed; they occur, however, where no mercury has been administered, as well as where it has. The symptoms alluded to are the most prominent symptoms of scrofula; and scrofulous children are very frequently affected with nodes, ulcers of the tonsils and fauces, and tubercular ulcerations of the skin, the equally prominent symptoms of venereal complaints. To make the analogy stronger, and, I will even say, their identity evident, let me here repeat, that they are curable, whether in children or adults, by the same mode of treatment. These circumstances are sufficiently enlarged upon in my essay, though our accurate critic thought proper to overlook the following passage on this part of our subject: "A defective digestion, continued for any length of time, must as necessarily produce chyle or blood of a vitiated quality, and unfit to replenish the waste of the body, as the constant use of unwholesome food, which is undergoing the putrefactive or acetous formentation. A disordered state of the system at first ensues, and is frequently succeeded by a variety of many local complaints; among these are swellings of the lymphatic glands, which frequently ulcerate; different species of cutaneous affections, particularly the tubercular eruption, which likewise ulcerates; and, lastly, ulceration arises in the throat.

Even the deep-seated parts do not escape, for the patient complains of pains in the joints, which are frequently mistaken for those of chronic rheumatism, and affections of the bones or their coverings, producing a kind of node, are not unfrequent. In fact, this train of symptoms occurs every day under our eyes, in children, whose youth fortunately protects them from the suspicion which lights on their elders; for it is by no means unusual to find in scrofulous children ulcers which arise from tubercles, and also ulcers of the tonsils, nodes on the shins, and pains in the joints, which, if they occurred at a more advanced period of life, would inevitably condemn them, in the hands of a great majority of practitioners, to a severe course of mercury."

With respect to the experiments of inoculation, in which the virus of cow-pock, and afterwards that of the smooth superficial ulcer was employed, and which the reviewer loudly deprecates as unfounded, irrational, dangerous, unjustifiable, wanton, and hazardous whims of the brain, unsupported either by analogy, reasoning, or any thing approaching to evidence,—I would beg of my reader to turn to my essay, and he will there find, or I am much mistaken, sufficient to convince the most untractable mind that they were well founded, rational, safe, justifiable, considerate, and promising dictates of the understanding, supported not only by analogy and reasoning, but by evidence amounting to absolute demonstration. These occupy fifteen quarto pages, and therefore cannot with propriety be repeated here, and any garbled extract would but weaken the force of their testimony. Suffice it to say, that the patients in question laboured under the tubercular eruption; that every remedy was tried for both which my own experience or that of my medical brethren could suggest; reiterated courses of mercury; reiterated cessations from its use; simple and compound decoctions of sarsaparilla; opium, Dover's powders, antimony, and the mineral acids; cessation from these medicines, and the enjoyment of country air; every thing was tried, and every thing failed. My reviewer, under my circumstances, would have abandoned my patients to their fate without an effort. It is true he suggests "that the most important step towards the cure of such patients as these who have been under many successive courses of mercury, is to get rid of all mercury from their constitutions;" and so unreservedly was I of the same opinion, that this "most important step" was taken and persisted in, and yet without any avail. Here, however, my reviewer would have stopped; but, I confess, I am not of that heartless and cold-blooded temperament, that can calmly and despondingly look on, while my patient may yet make a struggle for his life, and relinquish my exertions, to explore, on his behalf, the store-house of nature, for one of those thousand expedients that there lie unknown and unnoticed:—and it may not be unserviceable to others to learn, that, however hopeless the attempt might at first sight appear, however absurd, or even censurable, in the eyes of reviewers, the search has not always, thank Heaven! been in vain.

The reviewer may contrast, if he will, my expressions; he will not triumph long in cavilling at their meaning. Take for instance the following effort at criticism: "In one sentence the author calls this '*a rational conjecture*,' which 'struck him most forcibly;' and although in the next he characterizes it very justly as a 'strange proposal,' he carries it into instant execution." But let the reviewer reflect, that the man who first entertained the conjecture that inoculation might be an antidote to small-pox, entertained, as it since has appeared, a very *rational conjecture*; yet when it first was propounded it must have been considered as strange a proposal. Mine I avow to be both the one and the other. My reviewer admits it to be strange: I have only to convince him it is rational. He believes it to be a circumstance which yet remains to be proved, that more than one poison is suspended or superseded by the inoculation of the cow-pox. I fancy he may find some reason to change his belief, if he looks into Dr Adams's work on morbid poisons, * an author to whom I must here, as on other occasions, acknowledge myself indebted for most valuable information and useful suggestions. Besides the small-pox, both he and Dr Willan have witnessed the suspension of the hooping-cough and measles by means of vaccination. To proceed with the analogy, measles suspends the small-pox,—a fact witnessed by John Hunter, Mr Cruikshank and others. These poisons are in many respects analogous, and thence I inferred that other analogous poisons might also suspend or supersede each other. The venereal poisons, no one will doubt, are analogous; and if I inferred that similar consequences would be the result, there was still a strong and well known fact to uphold my conjecture—the frequent improvement and cure of obstinate venereal disorders of long and protracted duration, on the patients receiving a second infection, by means of venereal intercourse; yet I should not recommend a voluntary submission to this irregular mode of cure. The second infection may be worse than the first; or even if its nature was such as might be desired, still the ulcer might spread, and that which could do little mischief, however great its extent on the arm or thigh, might be destructive in its havoc on the glans on the prepuce. How much better then my suggestion, to choose at once both the place and the poison. The reviewer may puzzle himself again; and again put the question, "Is syphilis inoculated into the arm or thigh less virulent than when inserted into the glans or the prepuce?" I never said that it was; and if he chooses to set up a nine-pin of his own, for the mere pleasure of bowling at it, and which he has done more than once in his contest with me, I am not called upon to interrupt his amusement. All that is necessary on the present occasion to add is, that I tried the experiment, and I found my conjecture was rational;—the inoculated poison suspended for a time the influence of that which was in possession of the frame. And if I was not fortunate enough, on the very outset of the investigation to light on a venereal poison that would supersede, as well as suspend, the effects of its competitor, my conjecture was not the less rational, and will, I am convinced, in time be confirmed in the fullest extent. Meanwhile the evidence of both of the cases in question is decisive of this positive fact, that the poison of the smooth superficial ulcer is capable of suspending the poison of the tubercular eruption. When the former was exhausted, but not until then, the latter regained its dominion. But in neither of the cases was the experiment a second time tried. In one it was too late, in the other it was unnecessary. Phelan (case LXXX.) was seized with a dysenteric affection, which carried him off on the 23d of December, twenty days after the renewal of the original complaints had indicated the exhaustion of the inoculated poison. And Dunn (case LXXXI.) improved every day in his health; the ulcers had all but healed. I was looking forward to his discharge from the hospital, when, on the 31st day of March 1815, three months after the pains and papular eruption of the inoculated poison had disappeared, he also was carried off by a similar affection of his bowels.

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* See Adams on Morbid Poisons, 2d edition, from p. 12. to 16.

In another case of tubercular eruption to which I have alluded in my Essay, p. 202, I tried the inoculation of the syphilitic virus; but it had no other effect than to produce an ulcer which healed in six weeks, and no constitutional symptoms took place. We may therefore infer, if we can infer so much from one case, that the syphilitic poison neither supersedes nor suspends the tubercular. The patient is still in the hospital; his complaints growing worse, and his general health so far sinking, that it would only conduce to depreciate the value of any other experiment of the kind to try it upon him.

The reader is now in possession of every instance of venereal inoculation to which I have as yet found it necessary to recur. I have expressly stated in my Essay, p. 202, "that it is an experiment I would not resort to except in such cases only as resist every mode of treatment with which we are acquainted;" and I have the good fortune to be able to state, that the other modes of treatment with which we are AT PRESENT acquainted, have, since the publication of my essay, rendered it totally unnecessary to resort, or even think of resorting to *this*. Many cases of tubercular eruption, that most refractory and perplexing of venereal appearances, have been subdued by their means. None of them, it is true, were so obstinate as the cases we have just been discussing, and this favourable circumstance arose from the use of mercury having been, in a great measure, or perhaps altogether, avoided. It is peculiarly destructive in this species of eruption; and such instances as were from their commencement under my care, I found the least difficulty in overcoming, and that for no stronger reason that I can conceive, than because I most rigidly abstained from prescribing that mineral.

When I gave my work to the world, armed as it is in all its points by the stubborn strength of facts, I yet was not sanguine enough to believe that it would be received without opposition or cavil; and I knew that I was not to expect co-operation from those whose profession it is, with a few honourable exceptions, to pick holes where no flaw can be found, and to shew their acuteness and vigour, even at the expence of candour, good sense, and all critical morality. I therefore expected that one of my reviewers would take up one line of argument, and endeavour to persuade the world that there was no novelty in my views, and that all my newly discovered facts were perfectly well known to the profession before.

Another, I thought, would freely allow me all the merit of invention; but, as to any thing else but their novelty, it was very easy to say, that all my facts, my reasonings, and principles, were, *in fact*, good for nothing. But I never imagined that one and the same reviewer would take up the two incompatible arguments, and endeavour to convince the profession and the public that my views had been long known, long practised, and with long established success, and, in the same breath, announce that those very doctrines "were a premature grasp at generalization, and would not stand the test of experience, but prove to be at variance with more extended observation."

The following specimens of this mode of proceeding will suffice. I shall begin with those which detail the novelty of my views. "We have been however rather interested by the *novel* views of our author as far as they go, than instructed by his details, or convinced by his reasoning."—" Mr Carmichael's views, to which we must give the title of considerable *originality*, &c. many of his observations, especially with regard to the diagnosis of appearances, impress us with the character of great acuteness, which detects differences which it is not easy to communicate, and every part of the work evinces *that the author thinks for himself*."—" Although, therefore, we cannot follow Mr Carmichael as far as he has gone, we cannot but deem his statements as of considerable value to the profession, by calling the attention of practitioners to points of distinction which they are apt to overlook, and of *the existence of which multitudes in the profession are, we believe, ignorant*, and they evince on his part great zeal and activity of observation, and probably some *originality*."—" We wait with eagerness the appearance of Mr Carmichael's plates, and of the second part of his treatise; and in the mean time we consider the profession as indebted to him, for the *new* views of this complicated subject which he has presented to us, and doubt not that the path of investigation which he has opened, will lead to valuable knowledge, if it should not conduct us exactly to the objects which he has endeavoured to descry."

Who could suppose that the very same critical composition which contained the foregoing, should combine in its mixture such repulsive ingredients as the following. " So late as the summer of that year, we find him first discovering that these phagedænic ulcers, and certain other corresponding affections of the throat and limbs, were not relieved by mercury, *but were* aggravated during its use, and then first adopting *a new view* of the subject, *which, however, is perfectly familiar to experienced surgeons*."—" But as far as any truth is established, *less of real novelty* in the eyes of men of experience in this country, less in the estimation of those who have been educated in the school of John Hunter, or in the later schools of Pearson and Abernethy, than he appears from the hastiness of his publication to suppose." —" We believe, too, that in the eyes of the distinguished practitioners of experience in this country, very little that is *truly established in this treatise will be new*, and that *they could refute some of these novelties* by a host of facts of which we are not possessed."

I have now considered every item of any apparent value in these strictures; and shall merely state, that at present I do not claim any farther merit from my work than may rest on the honest statement of the cases which it contains. If others draw different conclusions from my facts, and can contrive to make it appear that I have been too hasty in forming mine, I shall not be backward in submitting to their better judgment, and publicly relinquishing opinions which I at present cherish most highly. Although my reviewer acknowledges "the perplexity and obscurity which prevail in the history of the diseases which resemble syphilis; the difficulty which every practitioner must feel in establishing any satisfactory principle of diagnosis betwixt them, and that even himself (*ourselves* according to the accustomed phraseology of reviewers) is very much in need of instruction on the subject," and even "remembers the difficulties expressed by one of the most learned and able surgeons and philosophical observers of the age, after an attendance of *thirty* years on the Lock Hospital of London, and the enjoyment of extensive general practice, respecting these spurious appearances,"—still seems to expect, that the man who attempts to discuss the subject is necessarily called upon to render it complete, and leave no more for others to perform, notwithstanding that "most of his cases bear no earlier date than 1812, two years only before the commission of his treatise to the press." Yet without doing all that is to be done, that I have considerably advanced the inquiry, I believe no candid person will deny; and if I did nothing more than point out so many means of discrimination which previously only consisted in the history of the disease, and the effects of mercury upon it, my efforts cannot be considered as uselessly employed. My predecessors, it is true, have done a vast deal, "but *not* more than *I* appear, from the hastiness of my publication, to suppose." For in that publication, hasty as it was, and recently as all those views it details were formed, I have, in the clearest and strongest terms, acknowledged all that I know to John Hunter, Mr Abernethy, and Mr Pearson, with whose "schools" the reviewer, rather shortsightedly, presumes me to be unacquainted. I refer him in the margin to my original acknowledgement;* but I consider myself more indebted to Mr Abernethy than to any other writer of past or present times. His observations first called my attention, not only to the subject under discussion, but also to those circumstances which gave occasion to my treatise on scrofula. † If he had not written before me, it is more than probable, that neither of these works would ever have entered into existence. These admissions, I trust, my reviewer will not overlook, as he overlooked those contained in my essay; but perhaps he did

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[*] See page 19 to 24—p. 51 and 208.

[†] I here allude to Mr Abernethy's "Surgical Observations on the Constitutional Origin and Treatment of Local Diseases."

not overlook them. I strongly suspect that these admissions, and others which I need not now advert to, were not only perceived, but silently seized upon, and then craftily turned upon myself, as if they had been originally started by the critical acumen of my reviewer. If I did justice to those who preceded me, I was not wanting in an endeavour to stimulate those who should succeed me in the task, to bring it to perfection. I never harboured the idea, nor thought of instilling it into others, that I had executed all that was to be done; but, in a plain and simple manner, I detailed the ample achievements of my predecessors, what fruits my humble exertions had succeeded in adding to theirs; and the concluding words of my essay, which I shall subjoin, will evince the earnestness with which I desired that others should also add their labours, and help to complete an enterprize half, and perhaps but half accomplished. "Far am I, however, from thinking that the subject requires no farther investigation. I trust that it will be pursued as anxiously as it ought by the great body of practitioners, until these diseases (as they certainly may) shall be thoroughly understood. Nor shall I consider that I have fulfilled my portion of this great duty, as long as any thing remains to be done, or the opportunities I have the good fortune to possess are still within my power.

Dublin, August 15, 1815.

VIII.

Cases of Reproduction and Reunion of part of the Finger after accidents; and Case of Absorption of the Water in Hydrocele, from mental agitation. By Edward Baynes Fletcher, Surgeon.

The two following cases are in some degree similar to the one recently communicated to the public by Dr Balfour; and as they may tend, in confirming the opinion of the public, on the possibility of parts being restored and reunited by the "powers of Nature," (though of considerable less importance than Dr Balfour's,) they may perhaps be allowed a place in your valuable Work. I have also subjoined a third case, which from its singularity, and as Dame Nature, aided by her assistant, Fear, were the only operators, may, though affording no practical knowledge, be deemed by you sufficiently novel to gain admission.

CASE I.

On the 24th of September 1810, Mr HEDLEY, carrier between Morpeth and Newcastle-upon-Tyne, I was then practising in Morpeth, came to my shop to get his finger dressed, the end of which was a few minutes before crushed off by a waggon wheel. On examination, I found the end of the finger, from the third joint to the point, including the nail, entirely cut off in an oblique direction, and hanging by a small portion of the cuticle. There was no injury of the phalanx. Not having sufficient confidence in the powers of nature to reunite the parts, I did what the surgeon in the blacksmith's case did, * divided the skin with my scissars, and contented myself with dressing the stump. Nature, however, was kinder to him than I was; for during the process of the cure, she restored to him, what I, in want of confidence in her power, had deprived him of. Granulations soon began to form and fill up the wounded surface; and though the process was slow, the finger recovered its original shape and appearance, but wanting a nail, which, however, in a few weeks, made its ground good. The lesson placed before me in this case, determined me, on any similar occurrence, to attempt to shorten the process, by endeavours at reunion,—a practice I had occasion to put into force last summer.

CASE II.

Master ———, then about 17 months old, playing near a heavy iron gate, which moved freely upon its hinges, by its suddenly falling to, got his finger completely split open from the second joint, from side to side. The upper portion of the phalanx was completely laid bare; the flesh and nail was merely attached to the under portion of the finger by a single small piece of cuticle. As I was standing beside him at the time of the accident, I immediately replaced the detached part in its proper situation, and secured it by adhesive dressings and bandage. Considerable inflammation of the hand ensued, but soon subsided; and as no particular fetor occurred, and as the child began on the second day to play about as usual, had good natural appetite and rest, I did not open the finger out till the sixth day, and had the satisfaction of finding reunion of the parts. The cuticle and nail were thrown off, but soon reappeared; and the hurt was scarcely observable.

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* Vide Note to Dr Balfour's Case, page 425, Vol. X.

CASE III.

Occurred to a patient of the late Mr Horn, of Newcastle-upon-Tyne, at the time I was pupil to that gentleman.

A stout robust elderly man, servant to a family in the immediate neighbourhood of Newcastle, was affected with hydrocele. Mr H. being the family surgeon, he came to consult him, who advised an immediate operation. This he was very unwilling to submit to. Finding, however, the tumour to get more troublesome to him, he at last consented. The morning was fixed; he came with a smiling countenance. Upon examination, the operation was found to be unnecessary; the fluid had been removed by absorption.

The following is (as near as I can recollect) the account he gave of himself: The night previous to the morning fixed for the operation, he went to bed labouring under great mental anxiety, "dreading the morning dawn." Fear prevented sleep. During the whole course of the night he perspired violently. Upon rising in the morning, he was both astonished and delighted to find he had no occasion to undergo the dreaded operation. I was in the habit of occasionally seeing the man for some years after this, and, so far as I know, he has never had a return of his complaint.

Carlisle, June 16, 1815.

IX.

Case of Reunion of the Thumb, communicated in a Letter to Dr William Balfour, Edinburgh. By THOMAS HUNTER, Esq. Surgeon, Port-Glasgow.

JANUARY 6th 1815.—JOHN GALBREATH, ætatis 45, by trade a house-carpenter, in the act of hewing wood with an axe, (which he held in his left hand) struck off his thumb, close to the articulation of the first phalanx, with the metacarpal bone. Being confused by the accident, he covered the stump with his other hand, and, accompanied by the foreman of the work, arrived about eight minutes after at my house. Upon examining the parts, I found the portion of thumb he had supposed cut off, lying in the hollow of his hand, buried in coagulated blood, and still attached by a portion of skin, not exceeding one-eighth of an inch in breadth.

Had I not been acquainted, through the medium of the Edinburgh Medical Journal, of your success in reuniting separated parts, I certainly, without the least hesitation, would have cast this part from me, and proceeded to dress the stump; but I resolved on saving it if possible.

Having carefully cleaned the parts, and removed a small splinter of bone, I replaced them, securing them with three stitches. After covering the thumb with dressing-lint, I placed a splint of wood above all, extending from the wrist to a little beyond the point of the thumb, and secured the whole, as neatly as possible, with a narrow ribbon. Finally, wetting the whole with Tinct. benzoin. comp. I ordered him to call frequently, that I might have the opportunity of ascertaining whether or not adhesion would take place.

I examined the parts daily. He felt no pain; no discharge took place; no smell was perceivable. In this way he passed the time without ever having the first dressings touched, till the twentieth day from the accident, when I became so anxious to see the parts, that I undressed the hand, and, to my great satisfaction, found all skinned, one place, where there was a stitch, excepted. This was dressed daily, and soon healed. The splint of wood was continued for some time after, to give support to the bone. The man has been working for some months past, enjoying all the advantages of his thumb, only the motion of the joint is impaired.

The above is a just statement of the case, and the patient signs it with the hand that suffered the injury.

(Signed) John Galbreath, *Patient.*
William Currie, *Foreman to the Work.*
Thomas Hunter, *Surgeon.*

X.

Case of Chronic Hydrocephalus, in which the mental faculties remained unimpaired till a short time before Death. By Dr Reed, Kilmarnock.

I take this occasion to communicate a case of hydrocephalus, in which the head had attained an extraordinary size. It shews the great changes that may take place in the structure of the brain, without materially impairing sensibility. W. H. was at birth a well-proportioned child. Certain nervous twitches and turnings of the eye-balls, however, seemed to indicate that all was not well within the cranium. These recurred during his whole life, though slight and distant. The child, meantime, increased in size, grew fat, and enjoyed excellent health and spirits. After three months, his mother found his head increase rapidly; his caps became very soon too small, and required perpetually to be replaced by larger. Things went on thus till he was three and a half years of age, when he had a severe fever; after which, although he perfectly recovered his health, he could never carry his head, which was now become very heavy and large. From this period, he either lay in bed, or on a kind of couch, made on purpose. It is proper to mention, that, owing to the weight of his head, he *never* could poise himself, so as to be able to walk. His faculties, however, expanded faster than is usual in children of his age; his vision was perfect; his hearing acute; his memory quick and tenacious; and his correctness in his childish affairs very remarkable. Various means were tried to lessen the size of the head, and a variety of medicines given without effect. Diuretics seemed for a time to arrest its progress, but soon lost their power. He lived till near five years of age, and was carried off by an epidemic influenza, January 3, 1814. His senses were perfect till within an hour of his death. Before opening, the head was measured, and its dimensions were as follows:

Round the head by the brow and occiput, 29½ inches.
From ear to ear over the forehead, 19 inches.
Round the head by the chin and vertex, 31 inches.
Water contained, 34 gills, (8½ pints.)

Nature had made great efforts to unite the bones by ossification; the deficiency was very considerable, and filled up by membranes. On puncturing this the water escaped to the extent above mentioned: and when it had all flowed out, the cranium resembled a large case thick lined; the cortical and medullary portions of the brain were squeezed together, and formed what seemed the lining of the cavity, and was about one-fourth of an inch in thickness all round. A few shreds appeared extending through it, in the direction of front and occiput, being, I presume, the remains of the partition between the ventricles. The rest of the cerebrum was absorbed; the cerebellum was seen lying at the bottom, healthy and entire.

Kilmarnock.

XI.

Case of a Child born between the Fourth and Fifth Month, and brought up.

By JOHN RODMAN, M. D. Paisley.

THE following case, I confess, is one of those which a sceptic will find little difficulty in contradicting.

The danger of being accounted credulous in requesting its publicity, however, need not intimidate me, for the truth was obtained, and the evidences were searched after as far as prudence could justify. My worthy friend Dr White, who has been highly respectable in this town as a medical practitioner for nearly forty years, and who is competent to judge in such cases, allows me to state, that he visited the mother along with me, saw the infant weighed and measured three weeks after birth, and assisted me to put the questions necessary for confirming the various statements. And it must also be mentioned, that the mother is more cautious in her decisions, accurate in her observations, and steady in her deportment, than what is usually met with in society, while the appearances of the infant were sufficient proofs to any one experienced in the practice of midwifery.

Being employed as accoucheur on the 19th of April 1815, when the mother, who had born five children, was confident that the period of her gestation was less than nineteen weeks, I found that her constitution was good, as well as her health; that she attributed the premature labour to fatiguing exertions on the preceding day; and had the happiness to deliver her of a living male infant.

Not daring to allow the washing of this infant's body, he was speedily wiped and wrapped in flannel, with only an opening in the dress around his mouth, for the admission of air; and by the time the dressing was over, the mother was ready to take him into the warm bed with herself. It is common, if there be much apparent weakness, to feed a child the first twelve hours after birth very frequently, yet, in this instance, although the child was weak, no feeding was attempted till beyond that time; the nourishing heat with the mother in bed was relied on.

On the following day, the head, body, and extremities of the child, were surrounded with fine cotton wool, pressed to appear like cloth, to the thickness of two or three folds, and over that the flannel, as before; and again the child was given to the mother in bed. His vital energy was so deficient, that, even with this dress, of himself he was unable to support the degree of warmth which was necessary to his existence. And no dependence was placed upon the heat of the fire; for, when that mode of heating him was tried, he evidently became weaker, whereas, the warmth of the mother in bed rendered him lively, and comparatively strong. Too much heat induced a sickly paleness of his face, with an obvious expression of uneasiness in his countenance; and the abstraction of heat, even by tardily undressing his head, brought on a nervous affection, or starting of the muscles all over his body.

From seeing how these morbid affections were induced, the child was kept regularly and comfortably warm, by the mother and two other females alternately lying in bed with him, for more than two months; after which, the heat of the weather being increased, and his vital energy being considerably augmented, he became able, himself, more and more to retain the proportion of bodily heat required, and they left him oftener to lie alone. When it was necessary to take him out of bed, and partly to undress him, (for he was never allowed fully to be undressed at one time,) every operation of this kind was quickly dispatched. On this account, neither the length nor weight of his body was ascertained till he was three weeks old, at which time the former was thirteen inches, and the latter, clothes and all, was two pounds and a half avoirdupois: the weight of the clothes was found afterwards to be eleven ounces.

It was extremely difficult to get the child to swallow nourishment the first week, and that which was principally used was toasted loaf-bread boiled in water, with the addition of a little sugar, and strained or passed through fine linen. He was frequently applied to the breast, in order to encourage the formation of milk, and also to give him a chance of some falling into his mouth. The yellow-gum soon came on, and the thrush seized him so severely by the eighth day, that his tongue, lips, and palate, appeared as if covered with films of coagulated milk; and as the portion of food he swallowed at any feeding-time was very small, like three, four, or six half tea-spoonfuls, ten to twenty drops of weak beef-tea were now added to such a quantity as this. The European castor-oil was likewise given him once or twice daily, from the quantity of six drops to that of nearly half a tea spoonful, and, without the use of other means, except a watery solution of borax, with which his lips were wetted a few times, and once that he got a little manna, the thrush was cured by the end of the third week.

This was the time he began to swallow tea-spoonfuls of his mother's milk freely; and in two days afterwards she was sensible of his exertions to suck. As these exertions grew stronger, the feeding system was gradually diminished, and by and by he supplied himself with his natural food. Yet the daily use of the castor-oil was continued, but not to purging, for two months longer.

Though he continued to suck, it was soon thought proper to give him a little of the fluid panada once or twice a-day, to the daily quantity of which ten drops of red Port wine were added. This plan is still followed, and the castor-oil is seldom necessary. He appears uncommonly happy after his dose of wine panada. But it should be pointedly recollected, that the panada is thin, that the quantity is small, and that every thing is done to him by the measurement of time. Of late he has been carried out occasionally in the open air; but the greatest care is taken to select the proper times of the day, and to regulate the length of time he is out. His age is nigh to four months, and his health and excretory functions are peculiarly regular.

In giving the history of this case, the conjectures of fancy have been disregarded, as well as the useless discussions of theory, and an attempt has been made to narrate the facts with simplicity. That the food which was given to the child was the best which could, in such a case, have been given, is not disputed. The beef-tea and wine were not prescribed from attachment to the speculative powers of stimuli, but from the experience of their beneficial effects in instances somewhat similar. Yet positive and indiscriminate prescriptions of this kind might lead into dangerous errors. It often happens that much good can be done by giving infants a few drops of red Port wine with precision, though the very contrary happens by giving it at random; of course, it is not merely the prescription and use of wine that is useful, the proper limitation of quantity is necessary, and the due degree of dilution, proportionated to the circumstances of the case. In a relaxed and irritable state of the bowels, this mode of treatment is often highly advantageous, along with emollient laxatives.

Considering how much the vital principle of young animals depends upon a proper degree of heat, it cannot be doubted that the power of cold is more or less destructive. The practitioner who has carefully attended to the way that sickly infants are guided by their keepers, must have frequently observed that the medical treatment has been defeated by the general conduct considered necessary by some keepers. The practice of keeping a child all night upon the knees of a nurse, when very ill, exposed on one side to the irregular heat of a fire, and on the other to occasional currents of air from the door, has been found to be highly prejudicial. In this way bowel complaints become obstinate; for, if parts of the child's body which are in contact with the nurse be overheated, the other parts are rendered acutely sensible to any cool medium, and, consequently, are liable to the chills which urge on the diseasing influence. At least, the bowel complaints of an infant are more speedily and effectually cured by remaining composedly in bed during an illness of this kind, than lying frequently on the knees of a nurse; and it is well known, that the temper of a child for submitting to what is necessary, can be wonderfully modified by perseverance in early tuition.

But an infant may lie in bed in cold weather and yet be injured by cold. Though vessels of warm water and such like be placed within the bed to increase the warmth, in this way it will be very variable, and all must allow that disease may often be the result of these variations. Hence it is, that I had found the natural and constant heat of an adult in bed with an infant, advantageous in several instances, and that the same plan was pursued in this case.

Paisley, 7*th August* 1815.

XII.

Remarks on the treatment of Epilepsy, and some other Nervous Diseases. By J. C. Prichard, M. D.

I consider the introduction of the free use of evacuant remedies into the treatment of nervous diseases, as one of the greatest improvements of the medical art which has taken place of late years. By drawing the attention of physicians to the beneficial effects of cathartics, in many distempers of this class, Dr Hamilton has performed a most important service to the public. I do not mean to undervalue his observations on febrile diseases, or to intimate that his method of practice is attended with fewer advantages in these complaints. But the old orthodox system of drenching with wine and brandy, in order to be before-hand with that dreadful goblin called typhus, which haunted so cruelly the physicians of the last age, had very much declined before the appearance of Dr Hamilton's book, and the inevitable result was, that the utility of evacuants, and particularly of cathartics, began to be known. The case, however, was very different with most of the nervous diseases, as they are called. In chorea, hysteria, epilespsy, &c. it would have been thought the height of absurdity to expect any benefit from active purging; and it was generally considered as contrary to the rules of the art to administer cathartic medicines in these complaints, except as mere laxatives. It is owing to Dr Hamilton, in a great measure, that these prejudices are now fast disappearing, together with many other similar canons of the old school, which, perhaps, are stained with as many crimes as the Spanish code against heretics. Imperfect as our knowledge confessedly is with respect to the pathology of nervous diseases, and inadequate as our remedies frequently prove themselves to be, we have yet the satisfaction of perceiving that we are evidently in the right path, and that, when we have not the means of cure within our power, we can, at least, often palliate, without incurring the risk of making matters worse than we found them.

Among the number of cases which appeared in Dr Hamilton's book, or which have been published subsequently, by other physicians, to illustrate the good effects of cathartic remedies, there are fewer of epilepsy than of almost any other malady which comes within the limits of Dr Hamilton's observations. I know not why this has happened, for my own experience convinces me that much advantage may be expected from purging in this disease; but, in order to supply the deficiency, I have taken the liberty of sending you some notes of a few cases which have occured to me, and request that you will, if you think fit, insert them in your excellent Journal. I beg leave to mention, that I do not flatter myself that there is any thing new in my method of treatment, but I think it probable that my remarks may, through the medium of your Journal, fall into the hands of some who, from habit, or an idea of the hopeless nature of the disease, may suffer cases to pass through their hands which are not altogether beyond the power of medicine, without attempting any active measures for their relief.

CASE I.

December 9, 1811.—ARTHUR CHARD, a sailor boy, about 15 years of age, of slender make, was seized, four days ago, with a fit while on board ship. He felt at first a peculiar sensation, attended with convulsive motion in one of his hands, which presently rose along the arm to his head. When it had reached his head, he instantly fell down senseless, and all his limbs were convulsed. He foamed at the mouth. After the fit went off he remained stupid, and affected with violent pain in his head. This morning he had another fit, which came on in the same manner, and was similar in every respect. Pulse 100, weak and small. The bowels had been constipated, but he has taken some laxative medicine since the first fit.

℞ Pulv. jalap. gr. xii.
Submuriat. hydrarg. gr. iv.
Ol. caryoph. gtt. i. M. ft.

Pulv. sumendus statim, et repetendus secundo quoque die.

December 13th.—Since he began to take the medicines, he has twice been alarmed by the previous symptoms of the fit. The sensation and convulsive motion in the hand came on as before, but ascended no higher than the fore-arm. No affection of the head or other symptom followed. He feels himself much better, but still complains of headache.

Repet. pulv. et singuli grana Jalapii xvi. contineant.

December 20th.—Has had no fit or previous symptoms. Is free from headache, and well in other respects. Let him continue the powders.

He continued some time to take the medicines, and had no return or threatening of his complaint until the autumn of the following year, when he came to me and told me that he had just had a fit. His bowels had been for some time constipated. I ordered him to take a cathartic powder every day, and he had no return of the convulsion. I have reason to suppose that he has suffered no attack since that time.

A curious circumstance in this case was, the sensation and convulsive motion in the hand which preceded the *attack*, and which may be considered as an example of the *Aura epileptica*. The Aura epileptica has been supposed to point out the seat of a local irritation, which excites the fit; and it has been proposed, accordingly, to divide the nerves in their course from the point where it arises, towards the head. But in this case, if there was any local irritation exciting the convulsions, it appears to have been situated in the intestinal canal; and the disease was removed by remedies which had no reference to the part affected by the aura. Hence we may be instructed to look for the exciting cause of the disease, in cases which are characterized by the Aura epileptica, rather in the general state of the constitution and principal functions, than in any local irritation in the part whence the aura arises.

CASE II.

June 23d 1815.—THOMAS HALLOWAY, aged 22 years, of a thin habit, and pale complexion, follows the business of a tiler, at Hambrook, a village near Bristol. He has been subject eight years to fits. He is suddenly seized with vertigo without any previous symptom, and falls down senseless. He is convulsed, and always remains without consciousness during half an hour or longer. After the fit has left him, he is troubled with headache and stupor during the rest of the day.

The first attack which he experienced, came on in consequence of a violent fright. During the early part of the complaint, the fits lasted longer, and were attended with more severe convulsions, and followed by more violent pain in the head, than since he has attained adult stature. At first, the fits occurred about once in three weeks. During the last three years, they have attacked him about once in a month, with nearly regular intervals; but, in the cold weather of winter, they frequently pass by the period of return. He has sometimes experienced even a longer interval than two months. His faculties seem to be unimpaired. The pulse is moderate and regular, and the appetite tolerably good. The bowels are regular.

I prescribed a cathartic powder, consisting of ten grains of jalap, as many of rhubarb, and five grains of calomel, to be taken every second night.

July 13th.—Perceives little alteration. The period of the fit has not passed.

August 7th.—The fit came on much as formerly. I find that he has not taken the powders regularly. Pulse rather frequent. He feels himself weak.

℞ Ext. colocynth. comp. ʒij.
Hydrarg. submuriat. ʒss.
Ol. caryoph. gtt. v. M. ft. massa divid. in pil. xxx.
quarum capt. iii alternis noctibus.
℞ Solut. sulphat. ferri rubri,
Tinct. digitalis āā. ℥ss. M. capt. gtt. xx. ter die.

August 29th.—He has taken the medicines last prescribed regularly, and has always been purged by the pills. Feels himself much stronger, and has a better appetite. He has had one slight fit in the regular period of return; it did not, however, deprive him of his senses, which has always happened hitherto. He was suddenly seized with vertigo, and a noise in his head, attended with nausea; but these symptoms left him without any further indication of the malady.

Finding that the fit observed pretty exact periods, I resolved to try the effect of arsenic, which seems to have a peculiar influence over diseases, which recur after regular intermissions. I ordered him to continue taking the medicines last prescribed, until within a fortnight of the time when he expects the next fit.

Sept. 20th.—Expects the fit in about a fortnight.

Repet. pil. ut antehac. alternis noct.
℞ Tinct. digital. ℥ss.
Solut. arsenic. Fowler. ʒij. M. capt. gtt. xv. ter die.

I did not see him again until November 29th, when he informed me that he had continued to take the drops prescribed, till within three weeks of that time. The fit which was expected, while he was taking the drops, passed over entirely. About a week after he left them off, he had a very trifling return of the peculiar sensations which used to accompany the fit; but it was in a much slighter degree than ever before. It amounted only to vertigo, and continued not a minute. He has persisted in taking the pills regularly every second night, up to the present time, (November 29th.)

I directed him to resume the use of the drops of the arsenical solution, and to continue taking the pills.

December 20th —He has had no symptom of the fits since I saw him last.—I ordered him to lay aside the drops and continue in taking the pills two or three times in the week.

January 10th, 1814.—He has had no return of the fits; is in perfect health, and much more robust than formerly.

July 1815.—I have good reason to believe that he has had no recurrence of his disorder.

In this case, it seems pretty evident that the purgatives produced a considerable effect on the disease, but were not alone sufficient to remove it. It is possible that their effect might have been greater if they had been continued longer. The arsenic seems to have afforded an important aid. *As for the* chalybeate drops, and the tincture of digitalis, they were not given in sufficient doses, or continued long enough, to produce much effect. In many cases of convulsions, attended with languor and a frequent pulse, I have found them beneficial.

Case III.

James Paynes, a blacksmith by trade, aged 27 years, came to me on the 29th of April 1814, complaining of fits, to which he has been subject eleven years. He is always attacked during his sleep, never in the day-time. In the night he is generally much disturbed with startings, and catchings of his limbs, and seldom enjoys quiet sleep. The fits have all the characters of epilepsy They return periodically, with an interval of three weeks; sometimes one fit is passed by, and the interval is six weeks;—this was the case before the last attack. For some nights before the fit seizes him, the sudden startings and catchings are more severe than usual, so that he always knows when the attack is about to come on. He is subject to headach and stupor. His appetite is good; his bowels generally inclined to be constipated; pulse natural.

℞ Pulv. jalap.
Pulv. rhei āā gr. x.
Hydrarg. submuriat. gr. v. M. sumend. statim, et post horas ii. Magnes. sulphat. ℥ss.
℞ Ext. colocynth. comp. ʒii.
Hydrarg. submur. ʒss. Ol carui gtt. v. M. ft. massa in pil. xxx divis. Capt. iii. omni nocti.
℞ Magnes. sulphat. ℥iss.
Mist. camphor ℥viij. M. Capt. cochl. ampla iii. ter die.

May 10th.—Little or no alteration has taken place; sleep much disturbed.

Perstet in usu pil. sumens iii. manè quotidie.
℞ Solut. arsenic Fowler gtt. v. ter die.

May 13th.—Just as before. Much pain in his head, and stupor.

Secatur arteria temporis et demantur sanguinis ℥xii.
Imponatur emplast. cantharid. amplum occipiti raso, et cutis postea irritetur ope unguenti sabinæ.
Capt. pilulas iii. omni nocte.
Perstet in usu magnes. sulphatis, et
Solutionis arsenicalis.

May 24th.—Finds himself much better; is free from stupor and headach; he has slept lately more quietly than he had done for several years; and although he has regularly continued the use of the evacuant remedies prescribed, he is stronger and more active than he was. Four weeks have now passed since he had a fit.

Let him continue all the medicines.

June 7th.—He is strong, and quite free from his complaint. Six weeks have passed since the last fit, and the period of return happens to-night, but he has no apprehension of being attacked, because the symptoms which always preceded the fits have not occurred; he sleeps with perfect tranquillity.

Perstet in usu medicam. et repet. emplast. canth.

He recovered from his complaint, and I have not heard of his suffering from any new attack, which I doubt not that I should have done if it had happened.

In this case I was induced to prescribe arsenic, by the same circumstances which led me to employ it in the instance of Holloway, viz. the regularly periodical return of the fits. It is evident, however, from the history of the case, that the evacuants chiefly contributed to the cure.

In the autumn of 1814, I had under my care the case of Margaret Campbell, a married woman, aged 23 years, of a florid sanguine complexion, and full pulse. When 15 years of age, she was attacked by a fit, in consequence of a sudden fright. The fit wholly deprived her of sensation and consciousness. From that period she continued to be subject to similar attacks. They seized her suddenly, without any previous symptom to indicate their approach. Sometimes they occurred several times in a day, and she never passed more than six or seven days without one. After suffering under these fits nearly four years, they left her, and she experienced no attack during three years; but, about 12 or 14 months before the time when I saw her, the complaint returned, with the same symptoms as before, and continued to trouble her from that time just as in the former period. During the fit she remained quite insensible, but was never convulsed. After it ceased, she was always affected for some time with severe headache, and frequently with nausea and vomiting; appetite and catamenia regular; bowels constipated. She is subject to ascarides.

I only prescribed a cathartic powder as follows:

℞ Pulv. jalap.
Pulv. rhei āā gr. x.
Hydr. submur. gr. v. M. ft. pulv. sumend. omni nocte vel alternis noctibus.

She continued the powders regularly during three weeks, at the end of which time I found her free from her complaint, and she has had no return. She discharged some ascarides after taking the first doses. She now enjoys perfect health, (July 1815.)

I must not omit to mention, that I have treated a considerable number of epileptic cases on the plan pursued in those which are above described, without obtaining such complete success. In many instances I have obtained only a temporary cessation, and in others have only been able to palliate the violence of the disease; but very few cases occurred which were not more or less relieved by the use of evacuants, particularly by active purgatives, local bleeding from the head, and blisters applied to the temples or occiput.

Before dismissing the subject of epilepsy, I wish to mention a suspicion which I have long entertained, that the accurate discrimination of convulsive diseases, as well as of many others, has been much impeded by the great desire which has prevailed of late years to simplify the nomenclature, and establish systematic notions concerning the nature and causes of distempers. I am persuaded that we commonly include under the name of epilepsy diseases which differ in important circumstances. I am not, however, prepared at present to propose an arrangement of them which may be satisfactory to the public or to myself, but have only to offer a few cursory remarks. I would propose to limit the appellation of epilepsy to those cases which are accompanied with signs of determination of blood to the head, and to distinguish those convulsive affections attended with loss of consciousness, which arise from indigestion, worms, and other irritations not immediately affecting the head, by Sauvages' old name of Eclampsia. I admit that it may be difficult to make, in all cases, this discrimination, but the attempt will lead to accurate observation. The circumstance on which this distinction is founded, and which it is intended to point out, is of great practical importance. In epilepsy, properly so called, there are evident signs of determination to the head. The appearance of the patient during the fit indicates it; and, in all the heads of epileptic persons which I have had an opportunity of examining after death, I have found a turgid state of the blood-vessels, and serous effusion. The effect of remedies also leads to the same inference; but, in the Eclampsia verminosa, and other similar affections, I think we can hardly suspect the same proximate cause, and our method of treatment should be varied accordingly. Perhaps a distinction of some importance may arise from the circumstance, that some convulsive fits occur always during sleep, others while the patient is awake. A remarkable variety of the disease are those fits of the epileptic character which attack women at or about the period of menstruation. I have tried the use of arsenic in this form of periodical epilepsy, but never obtained any success. In one case the fits were absent for two or three months, but the catamenia were also interrupted, and when they returned, the fits returned with them.

I have tried the use of evacuant remedies in several other diseases of the class Neuroses, with success, but in none with more singular advantage than in mania, in which distemper I have had extensive opportunities of witnessing their effects, having been for some years one of the physicians to an hospital where a great number of lunatics are admitted. I am firmly persuaded, that, if medical practitioners would treat this disease under the idea of its being of an inflammatory nature, and allied to phrenitis, and would depend more on physical, and less on what they term moral remedies, they would succeed in a greater proportion of cases, especially of recent ones, than generally happens.

I shall conclude these remarks by briefly enumerating the indications in which it appears to me that cathartic remedies are chiefly serviceable.

1. By removing sordes from the intestinal canal. The very extensive use of cathartics which is sometimes necessary in this indication, was first pointed out by Dr Hamilton. Although I have seen this practice pursued to a great extent in the Royal Infirmary of Edinburgh, and have been in the constant habit of using cathartics very freely in my own practice, I am every day surprised at the prodigious accumulations of feculent matter which I find to take place in the intestinal canals of patients of all ages.

2. As depleting the system. In this view the habitual use of purgatives is more important than their effect in any case of emergency.

3. As determining the fluids from the head. The effect of a strong cathartic in inducing syncope and paleness of the face, is a proof of this power in purgative remedies. The quantity of fluid evacuated from the system cannot produce these phenomena.

4. As setting up a new action in the system. Although this proposition has a hypothetical aspect, I am convinced that it is the principle upon which the beneficial effect of cathartics often depends. Having, however, trespassed already too long on the patience of your readers, I shall not attempt at present to explain my reasons for this opinion.

5. Purging is a powerful means of stimulating the absorbent system, as we witness in its effects on dropsical patients.

6. A course of moderate purgatives is one of the most efficacious methods of invigorating the disgestive organs, improving the appetite, and removing visceral obstructions.

Since these remedies are subservient to such a variety of indications, it is not wonderful that there are few diseases in which their use does not form an important resource of the healing art.

Bristol, July 8, 1815.

XIII.

Case of painful Subcutaneous Tubercle. By Marshall Hall, M. D. formerly Senior President of the Royal Medical Society, and Physician's Assistant Royal Infirmary, Edinburgh.

I transmit you the particulars of a case of the Painful Subcutaneous Tubercle, of which Mr William Wood has given so excellent an account in the Edinburgh Medical and Surgical Journal for July and October 1812. The intelligence of my patient has furnished me with a variety of circumstances which may add to the history of this affection in general. The case affords another instance of the occurrence of the subcutaneous tubercle in the male subject; it presents an example of severe and painful suffering continued during the long period of two and twenty years; and it illustrates the perfect safety and efficacy of the cure by excision.

In 1793, Mr Edmund Hart, Low Pavement, observed a slight bleeding from nearly the point of the fore-finger of the left hand, and being a shoemaker, he suspected that he might have punctured this part with the awl. Some time after this bleeding had ceased, an acute pain was experienced in the point of the finger, on putting the hand into the pocket, which contained some halfpence; and after this period the patient was always affected with very acute pain on touching any hard substance. Nothing was at that time observable on examination; at length, however, a small red speck was discovered within three lines of the point, and on the ulnar side of the finger. Soon afterwards, *paroxysms* of pain, unoccasioned by external injury, began to be experienced, during which a little redness and tumour were observed in the part affected.

About seven years after the commencement of this affection, the paroxysms of pain became more and more severe, and came on nearly regularly every fortnight, during the course of the *day*. At this period an application of caustic was made with temporary relief. Some time afterwards, the patient resorted to the practice of paring away a small portion of the integuments over the spot, so deep as to make the part bleed a little. This he did from time to time, and always with a certain degree of relief, probably by rendering the part less prominent, and consequently less exposed to external injury.

From the period last mentioned, the paroxysms of pain became more severe and more frequent, until at length the pain, in a more or less exasperated form, was nearly constant. About eight years ago this pain began to be particularly urgent during the *night*; and during the last two years a great part of each night's rest has been lost from it, an aggravation of the pain taking place about eleven and continuing until three o'clock in the course of each night. The part affected itself has suffered little change and little increase in size. It has been observed to be rather more tumid and red in the evening than in the morning, and during the paroxysms than during the absence or alleviation of the pain.

In the paroxysms the pain became more and more severe by degrees, and was accompanied by a painful sense of pulsation. This pulsation was irregular, and not experienced at each pulsation of the arteries. The pain extended to a considerable distance from the point affected, along both sides of the arm and along the middle-finger; it was never felt in the thumb nor in the other fingers, but sometimes the back of the hand and the root of the little-finger on the outside were affected. In moments of acutest suffering he experienced pain down the left thigh and leg to the toes. Occasionally, on touching any hard substance, the pain has been so excruciating as to induce fainting.

The patient's suffering has always been particularly aggravated during any general indisposition, once during the continuance of a febrile affection from an accident, and at all times during the presence or aggravation of certain dyspeptic symptoms to which he has been subject. The pain was also aggravated during the prevalence of cold or wet weather. On washing the hand in cold water in the winter season, the pain was immediately induced, and the patient was obliged to expose the part immediately to the warmth of the fire for relief.

Besides the application of caustic, and the removal of a small portion of the integuments already noticed, the patient has experienced temporary relief from the application of adhesive plaster, of a blister, and of spirit of wine; and, during the paroxysms, by exposure to considerable warmth near the fire.

Such is the history of the case to the day on which Mr Higginbottom and myself were consulted by Mr Hart. *On examination* of the finger, a small hard red spot was observed about three lines from the point of the finger, on its ulnar side. The integuments were rather changed and immoveable over this hardness, probably from the different modes of treatment which had been employed. The spot was exquisitely sensible on rubbing or pressure, but the parts around possessed the ordinary degree of sensibility. The pain on pressure seemed to dart along the course of the nerve to the radial side of the middle finger, and upwards along the arm.

The patient earnestly requested the amputation of the first joint of the finger. But it was decided that Mr Higginbottom should extirpate the tubercle by two free curvilinear incisions. This was accordingly done. The wound healed in a few days; the pain ceased from that moment, and the patient has experienced no subsequent uneasiness of any kind.

I mention this last circumstance particularly, because the patient has been alarmed by the suggestion that there would be a danger of the supervention of trismus from any operation. The result of this case has, however, only tended to confirm the observation of Mr Wood, that "the tubercle may be extirpated with perfect safety, no unpleasant symptom having been ever produced by the operation." *

The small portion of integuments removed was divided by the scalpel after the operation: they were rather indurated over a small body of the texture of cartilage, which seemed to be contained by them as in a cyst, from which it was expressed by the thumb and finger. The portion of integuments removed was too inconsiderable to admit of tracing the small tubercle or its cyst to any connection with the nerve or blood-vessel of the part.

The patient's conjecture relative to the cause of his complaint is probable. There is a case of punctured nerve, somewhat analogous, detailed by M. Verpinet, in the third volume of the Edinburgh Medical and Surgical Journal. The subcutaneous tubercle occurs more frequently in women than in men, probably from the use made of pins in their dress; and it is possible that the Tic douloureux, which is allied to this affection, often arises from the injury done to the nerves of decayed teeth, or the laceration occasioned by their extraction.

Nottingham, August 20.

XIV.

Case of Disease in the Brain, with Pathological Observations. By Thomas Salter, Member of the Royal College of Surgeons London. (*With an Engraving.*)

George Langdown, sailor, 35 years of age, of a sanguineous temperament, in March 1813, waded up to his waist in the water, to unmoor a boat made fast to a post at a considerable distance from the shore. It was in the early part of the day; and, notwithstanding he felt cold and uncomfortable, he did not change his clothes. In a day or two he complained of pain in the head; first over the right ear and temple, then in the forehead. In a short time the pain diminished in these parts, and became particularly severe at the back part of the head, nape of the neck, and between the shoulders. These symptoms he attributed to taking cold from the above imprudent conduct. The pain often varied in degree; though it never wholly left *him*. He had occasionally vertigo, and would often suddenly become faint, and sick, but vomiting never took place. I first saw the patient, April 28th, when I received from him the above account. The pain was, at that time, often distressing, referred chiefly to the occiput; the eyes had a heavy aspect, and the vessels of the conjunctivæ were turgid, but the irides were obedient to their natural and healthy laws.. The bowels were habitually slow, and have of late been increasingly costive; appetite good, pulse eighty, and rather full. A pound of blood was drawn from the arm, and cathartic pills, containing a portion of the submuriate of mercury, were ordered to be taken every night, to regulate the action of the intestines. The patient was also directed to eat sparingly of animal food, and rigidly to avoid all kinds of fermented liquors. From this time he ceased to be under my care; though I afterwards once saw him, and have been informed he did not experience much benefit *from the* above treatment, and, as is too common, when relief does not immediately succeed to the trial of any prescribed remedy, without following in a fair manner the advice proposed, he sought the assistance of another practitioner. However, from the circumstance of his allowing two or three months to elapse before he did so, I am inclined to believe the symptoms were somewhat mitigated. In August he was attacked with hemiplegia of the left side. Feb. 1st 1814, I accidentally saw *him* again, and noted the following observations: The palpebræ are generally more than half closed. The upper lids are equally incapable of elevation, and of being approximated to the lower ones, and remain in the same position asleep or awake. His vision is very imperfect, and is principally confined to the left eye, the retina of the right being nearly insensible to the impression of light; of which a sudden and strong application does not cause its pupil, which is much dilated, visibly to contract. He can see distant objects much better than those which are nearer. In impinging upon the retina, so as to produce distinct vision, the rays of light must come obliquely from the external canthus; consequently the axis of vision of the left eye is directed towards the temple, and the paralytic state of the palpebra require it to be elevated with the finger, at the same time that the hand is held over the forehead, to moderate the impression of light, which, if strong, always occasions pain. The patient has in a measure recovered from the paralysis of the left side. Though at this time the pain in the head is very considerable, it is less so than formerly. He is often giddy, and has disagreeable noises in his ears; is also much disturbed in his sleep by frightful dreams; and, as his friends inform me, his temper is lately become exceedingly irritable. The bowels are extremely torpid. The character of the feces has not been ascertained, nor has any thing unusual been observed in the urine or the action of the urinary organs. Appetite tolerably good; pulse 60, and weak. After this, being under the necessity of seeking parochial assistance, he fell under the care of a third practitioner; who has obligingly communicated to me an account of his situation during his attendance upon him, which ended with the death of the patient. By the above gentleman it appears, that, whilst under his care, the pain was much less distressing than in the early part of the disorder; though, as before observed, it never did entirely leave him. He was now troubled with fits of stupor, sometimes twenty in the course of the day, each upon the average lasting about four or five minutes. He was apprized of their approach by pain and vertigo, and would call some person in attendance to hold his head. This it seems afforded him considerable relief. All consciousness was lost during the paroxysm, but when it had ceased he was reminded of its commencement, by reflecting on the pain and distressing sensations by which it was ushered in. When the fit was about to terminate, a profuse perspiration usually broke out upon the face, and upper part of the body. His memory was remarkably defective, and his temper exceedingly irascible. In the right eye he was destitute of sight, and saw but very imperfectly with the left. His hearing did not appear to be much impaired, but he had universal muscular debility; and, from May 1814, ceased to possess any power over the lower extremities. The pulse never exceeded sixty, even under the influence of cordial remedies; indeed it often fell below forty, and was frequently so weak as to be nearly imperceptible at the wrist. The extremities uniformly cold, and sensation in them much impaired. The appetite was good, until within a few weeks of the patient's death, and the process of digestion seemed to be pretty well performed. The bowels were excessively costive, requiring the most drastic purges to stimulate them to action, which appears to have arisen more from want of muscular power in the intestines, than from a defective or altered secretion of bile, as the feces were not otherwise unnatural, than being of an unusually hard or firm consistence. There was a paucity in the secretion of urine, yet its quality was not incompatible with health; in passing it, difficulty was frequently experienced, though the bladder never wholly lost its tone. Respiration was generally free and easy, till a short time previous to the patient's death, when it was interrupted by spasm and a sense of suffocation. About a fortnight before he died, the small degree of power was lost, which he had till then retained over the upper limbs; after this he declined very rapidly, and died October 14, 1814. Twenty four hours after death, I opened the head. The following is an account of the appearances presented by the contents of the cranium. The dura mater, although it was preternaturally vascular, did not adhere very tenaciously to the calvaria. The longitudinal sinus was slit open, but there was no disease. It contained a small quantity of coagulated blood. The glandulæ Pacchioni were fewer, and the chordæ Willisii less evident than are commonly met with. There was a greater effusion of fluid between the tunica arachnoides and pia mater, than I ever before remember to have seen; the quantity was particular great at the base of the brain. The fluid was milky and opaque; and threads of what seemed to be coaguable lymph were here and there mixed with it. If I had wished to demonstrate the arachnoid membrane, a more favourable case could not have occurred, as several square inches of it were easily detached. The vessels of the pia mater were gorged with blood, and between their ramifications, the *vermillion* tint of inflammation was almost everywhere to be seen. On cutting the substance of the cerebrum, the points of blood on the surface of the incision, which in the healthy brain are few and small, were here uncommonly large and numerous, and the colour, both of the cortical and medullary matter, was darker than natural. The brain was soft, though not so much so as in hydrocephalus. There was about half an ounce of fluid in the ventricles. The plexus choroides and velum interpositum were remarkably turgid with blood. The optic nerves were healthy.

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* Edinburgh Medical and Surgical Journal for July 1812.

It remains, however, to describe the most remarkable part of the disease, which was principally seated in the pons Varolii, or tuber annulare. This body had undergone great change in structure. A considerable portion of it was converted into a substance of a fleshy appearance, forming on its surface two triangles, with their apices nearly united; the base of one extending some way into the right crus cerebri; that of the other not only as far as the commencement of the medulla oblongata, but a little way upon it. The diseased part was not elevated above the general surface of the tuber annulare, but penetrated its substance to a considerable depth. It was very firm, and somewhat fibrous, though it cut much harder than muscular fibre. When detached from the other parts, which were soft and flabby, and easily squeezed from the diseased portion with the fingers, it was found to weigh two drachms. To preserve it, I put it into muriatic acid, at the same time I doubted the propriety of the use of this fluid, considering how widely different the parts were, not only from healthy brain, but also from that organ under any disease with which I had been before acquainted. However, I must acknowledge myself to have been surprized, on finding, in a few hours, a great deal of it dissolved, and that, by the next day, the whole had undergone complete solution, the acid remaining clear and transparent as before, except being a little heightened in colour.

With the exception of colour, the annexed engraving gives a correct representation of the diseased tuber annulare. The dark parts, marked *a*, *b*, are the two triangular appearances before described. It will be observed, that the pons Varolii is broader and flatter than usual, which is owing to the soft and flabby state of what might comparatively be called the healthy portion of it.

Observations.

In the present enlightened state of the profession, to the inquirer after medical science the study of morbid anatomy is justly thought of the first importance. It certainly must be allowed to be of great practical utility, as no person can be considered as fit to combat disease in an organ, of the morbid changes in the structure of which he is ignorant. The brain, of all the organs of the body, from its peculiar structure, and I may perhaps with truth say, from its incomprehensible functions, presents the greatest obstacles to a thorough knowledge of its pathology. It becomes, therefore, highly necessary that every organic derangement to which it is liable should be accurately recorded. Impressed with this notion, I have been induced to publish the foregoing case, which other motives, arising out of its obvious and unavoidable imperfections, would have prompted me to withhold. The chief diseases of the encephalon treated of by authors, are inflammation and ossification of the meninges, —scrofulous tumours, generally in the substance of the brain, and sometimes, though rarely, attached to the membranes and plexus choroides,—hydatids,—abscess,—hydrocephalus,—and apoplexy. But I have nowhere met with a description of a case which appeared to have any similitude to the one forming the subject of this communication. The peculiar disorganization of the tuber annulare could not be the effect of common inflammation of the substance of the brain, * though it undoubtedly was the result of an increased action of the vessels; that action, however, was of a specific kind. The complete *solubility* of the diseased part in muriatic acid, is a singular circumstance, and a striking proof how greatly it differed from the healthy structure. For some months previous to the patient's death, the inflammatory state of the membranes was most probably of a chronic nature; and the turgescence of the vessels observed on dissection, more the effect of weakness in their coats, than of an increased action of the vessels themselves:—weakness, occasioning an inability in them to contract, by which the same quantity of blood was circulated as in the active stage of the disorder; in fact, a state no way differing from chronic ophthalmia. The diminution of pain which the patient latterly experienced, and the protracted termination of the case, may be considered as sufficient evidence of the justness of this conclusion. The effusion of fluid upon the surface of the pia mater, was the natural consequence of this state of the parts. The paralysis, as might be expected from the known laws of the organ, was on the contrary side of the body from that of the brain in which the disease was situated; at least, it was so until the morbid condition of the parts had extended towards the other side of the pons Varolii, and the whole contents of the cranium had become more or less deranged when the paralysis was more universal. I shall not attempt to explain the cause of the amaurosis of the right eye. The subject is too intricate; and, indeed, in the present divided state of opinions, whether the optic nerves decussate, or barely unite at the sella turcica, it is not likely that any thing satisfactory can be advanced on the subject. If, in the present *instance*, so far as an individual case goes, the diseased state of the right crus cerebri should by any person be considered as the cause of the amaurosis, it would seem to favour the opinion of the decussation of the nerves. For if the other cerebral nerves, which are unconnected, and have obviously a straight forward course, be affected by an injury of the opposite side of the brain, the crossing of the optic nerves will necessarily counteract that effect; and the amaurosis, as well as the injury of the brain, will both be found on the same side. The situation of the disease satisfactorily accounts for the pain being chiefly referred to the occiput, and the debility in the muscular coat of the intestines, which it must have experienced in common with the whole muscular system, arising from defective nervous energy, may well

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* " The part which is inflamed has no peculiar hardness, but yields nearly

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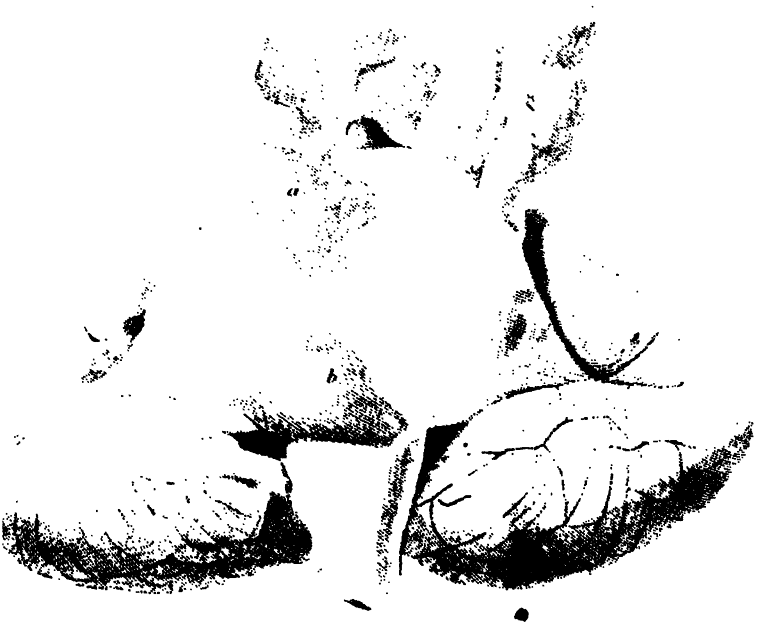
the same sensation to the touch as it would do in a healthy state."—Baillie's Morbid Anatomy, 2d edit. p. 432.

Mr Walter's case of Diseased Brain.



Published by A. Constable & C. Edinburgh 1815.

explain the reason of the obstinate constipation. This opinion is the more probable, as the feces were properly tinctured with bile, and therefore constipation could not have arisen from a deficiency in that secretion.

I must regret, that the ignorant prejudices of the patient's friends would not allow the abdomen to be examined, as I thought it a case likely to throw some light upon the subject of that reciprocal morbid sympathy which some distinguished physicians have supposed to exist between the brain and liver, and which I believe to be an opinion founded both on just principles and accurate observation. At the same time, I think, that, while the attention of the profession has of late been so much directed to the latter organ, the effects of diseases in the other viscera upon the brain, have not received that investigation which their importance would seem to merit. I was led to this conclusion, from the consideration of the origin and distribution of the abdominal nerves, * and from having lately had an opportunity of examining, in my own practice, and in that of others, four patients who died of hydrocephalus internus, in all of whom the liver was natural. In the two first there was no apparent disease in the abdomen. In the third, the spleen was greatly inflamed, and full of hard white tubercles, about half the size of peas. It adhered so firmly to the diaphragm, that its substance was torn in detaching it. The state of the spleen, in the fourth case, differed in no respect from the preceding, but the lungs were, I think, still more loaded with tubercles than the spleen, though many of them were smaller, resembling millet seeds. These were decided cases of hydrocephalus internus, such as no one at all, accustomed to see the disease, could mistake.

I also lately dissected a case of apoplexia, where the liver and all the other abdominal viscera were found in a healthy state, though the patient had, for a long time previous to his death, been subject to a disordered state of the digestive organs. This, and the two first cases of hydrocephalus before mentioned, would seem to warrant an opinion, that it is not at all times necessary to give rise to sympathetic disorganization in the brain,

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* The great sympathetic nerves, whilst in the thorax, send off the rami splanchnici, which pass between the crura of the diaphragm into the abdomen, and form, in conjunction with some branches of the eighth pair of nerves, about the root of the cœliac artery, the great semilunar ganglion. A mesh of nerves proceeds immediately from the semilunar ganglion, denominated solor or cœliac plexus, from which separate plexuses are sent, accompanying the arteries to all the viscera of the abdomen, and obtain their names from the respective viscera they supply.

that actual diseased structure should exist in the abdominal viscera, and that sometimes a deranged action of the organs alone will be sufficient to produce that effect. A case but a short time ago occurred to me, which I think demonstrates the fact; and, as it appears to me an important one, I may, perhaps, whilst on the subject, be excused the additional length it will give this article, by only briefly relating it.

Mrs ———, a lady 49 years of age, rather below the middle stature, fair skin, dark hair, had been troubled for many years with dyspeptic and nervous complaints. The integuments had that peculiar sallow hue which usually attends cachectic diseases. Her indisposition had created a particular dislike to animal food, and, for many months previous to her death, she refused it altogether, eating only, in a very sparing manner, of solid aliment of any kind, and subsisting almost entirely upon milk and water. The bowels were in a very irregular state; sometimes confined, at others greatly relaxed; stools seldom healthy; often dark and extremely fetid, though mostly green and slimy, resembling the alvine evacuations of children suffering under constitutional irritation. The urine was secreted in proper quantity, but uniformly pale, and without sediment, not unlike the urine of the hysteric paroxysm. There was often pain in the back between the scapulæ, and tenderness, with a degree of pain, in the epigastrium; there was also a fulness in the right hypochondrium, though the liver could not be distinctly traced below the margin of the ribs; pulse quick and feeble, rarely below 100; tongue mostly clean, but chapped. This may be considered as a correct account of the patient's state, (the last fortnight excepted) for at least ten months before her death. She took the blue pill, and a variety of vegetable bitters, combined with the fixed and volatile alkalies, which, as long as they were continued, never failed to correct, in a considerable degree, the morbid condition of the abdominal viscera, and to alleviate all her symptoms. About a fortnight before her death, another and a more alarming train of symptoms made their appearance. The sickness, which was before only occasional, became now almost continued, though nothing but a little frothy mucus was brought up. She complained of indistinct vision, and was very lethargic; respiration became slow, not averaging more than twelve in the minute. The pulse also fell to eighty. She manifested the greatest disinclination to take any kind of sustenance, much persuasion on the part of her friends being often necessary to induce her to make the attempt to swallow. Scarce any urine was now secreted, but its character remained unchanged. There were tremulous motions of the voluntary muscles, especially when an attempt was made to exert them. Her skin was also at this time universally affected with prurigo. These symptoms continued with but little variation, until a few hours previous to the closing scene, when, as is usual in diseases of the brain, the pulse again rose, the artery beating an hundred in the minute. In about twenty-four hours after death the body was examined. There was much fluid between the convolutions of the brain, under the tunica arachnoides. The veins of the pia mater were turgid, and numerous bloody points dotted the cut surface of the cerebrum. Two ounces of fluid were found in the lateral ventricles. The plexus choroides was pale, and the substance of the brain was very soft. On opening the abdomen, with an expectation to find disease in that cavity, the liver first struck me as being enlarged; but as no diseased appearance was to be seen on any part of its surface, and as sections of its substance displayed no morbid state of the internal structure, I entirely gave up the notion, and consider that viscus to have been in the most natural state. The gall-bladder was two-thirds full of healthy bile. In the villous coat of the stomach were a few of those vascular spots described by Dr Yellowly in the fourth volume of the Medico-Chirurgical Transactions, and which that writer does not consider as a mark of disease. * I had often observed this appearance in the inner membrane of the stomach in persons who died of different diseases; and once in the greatest degree in a woman who was burnt to death, and who had no previous complaint, which I was unable to account for, until I had read that learned gentleman's ingenious paper upon the subject. All the other viscera were natural.

Poole, June 16, 1815.

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* "They occur in every variety of degree and character, under every circumstance of previous indisposition, and in situations where the most healthy aspect of an organ might be fairly expected."—Medico-Chirurgical Transactions, Vol IV. p. 388.

PART II.

CRITICAL ANALYSIS.

I. II. & III.

1st, *A Treatise on Hydrencephalus, or Dropsy of the Brain.* By JAMES CARMICHAEL SMYTH, M. D. F. R. S. &c. London, 1814. 8vo. pp. 120.

2d, *A Second Essay on Hydrocephalus Acutus, or Dropsy in the Brain.* By J. CHEYNE, M. D. F. R. S. E. M. R. I. A. Professor of the Practice of Physic in the School of Surgery in Ireland, &c. &c. &c. Dublin, 1815. 8vo. pp. 74.

3d, *A Statement of the Early Symptoms which lead to the Disease termed Water in the Brain; with Observations on the necessity of a watchful attention to them, and on the Fatal Consequences of their neglect: in a Letter to* MARTIN WALL, Esq. *M. D. Clinical Professor at Oxford, &c.* By G. D. YEATS, M. D. of Trinity College, Oxford, &c. &c. London, 1815. 8vo. pp. 114.

IN the greater number of those who have died after suffering that series of symptoms which constitutes this interesting, and still too fatal disease of childhood, from one to four or six ounces of serous fluid have been found effused in the lateral ventricles of the brain. This was the first, the most obvious, and the most undeviating appearance which presented itself to the observation of the medical anatomist; an appearance which could scarcely fail to suggest and to justify the notion, that this disease was indeed a dropsy seated in the brain, and that all the symptoms observed, from the first invasion of the disease to its fatal termination, were caused by the effusion of this fluid, and its consequent pressure on the sensorium.

This simple view of the pathology of hydrocephalus acutus, could scarcely suggest to the physician any other curative indication than the very unpromising one of accomplishing the absorption of the dropsical fluid. The publication of Dr Dobson's successful treatment of a disease, which had before proved so generally fatal, by the free administration of mercury, raised indeed a hope, at once general and confident, that hydrocephalus might be made to yield to the powers of that active mineral. Mercury accordingly became the universal nostrum and panacea in this disease. Some, indeed, out of the hundreds who continued to perish, have been recovered, or seemed to have been recovered, by the mercurial treatment. But the universal, indiscriminate, and unsuccessful application of the mercurial unction in this disease, has too much resembled the extreme unction of the Roman church.

In the progress, however, of observation, it was remarked, that in some cases little or no fluid had been effused within the brain of children who had died of this disease; and that in others the effusion of fluid was not the only appearance presented on dissection; that there were signs of congestion, and of vascular excitement in the brain and its membranes, or of inflammation, and its consequences,—adhesions, coagulable effusions, and thickenings. These, and such observations, were well calculated to inspire a doubt of the truth of that pathology, which, overlooking those other morbid changes, referred the whole phenomena of the disease to the small quantity of fluid found to be effused into the ventricles of the brain; and accordingly Dr Quin, and afterwards Dr Rush, enlightened by these observations, were led to a very different, and, we apprehend, a more just view of the subject, which considers the serous effusion, when present, as a consequence or effect of an antecedent excitement, inflammation, and congestion. All the appearances observed on dissection thus received a simple and more consistent explanation, and the phenomena which characterize the different stages of the disease, a more connected solution. A better prospect, too, was opened to the practical physician; for a hope was now entertained, that, by a prompt antiphlogistic treatment in the first stage of the disease, the serous effusion, and other disorganizing effects of inflammation and congestion, might be obviated and prevented. Physicians readily adopted this reasoning; and the disease, no longer regarded as a mere dropsy of the brain, had the new name of Phrenicula bestowed upon it. The propriety of this change of nomenclature is indeed very questionable. Yet the general doctrine, that the serous effusion within the brain is not the primary morbid change which takes place in this disease; that it is the consequent effect rather of some preceding excitement, or morbid change, we hold to be true in fact, and important in its practical application. It has given to the physician confidence in the practice of evacuations and counter-irritations, which have undoubtedly proved beneficial when employed in the early period of the disease, and especially in that variety of it which attacks the patient suddenly and acutely; and it has directed his attention to the other antecedent derangements in the health of those whom hydrocephalus may be said to attack more insidiously.

Those symptoms of deranged health are in a remarkable manner observed in the functions of the digestive and chylopoetic viscera. Many of the symptoms which constitute the disease called hydrocephalus, occur in children who have worms in the primæ viæ; and the diagnosis between the remittent or abdominal fever of children, and hydrocephalus acutus, is in many cases difficult and doubtful. There is hardly, indeed, a symptom of hydrocephalus, which may not also be observed to occur in cases of worms, and of infantile remittent fever; and it is rather by a nice attention to the series and order of the occurrence of the symptoms, and to the mode in which they are grouped, than by the exclusion of any one of them, that we can establish their diagnostic differences. Besides, in many cases of hydrocephalus, the approach of the disease is insidious, the more decided features of the disease being preceded, for weeks, by a sort of chronic cachexia, with symptoms referable to disordered function of the chylopoetic organs, rather than of the head. In hydrocephalus itself, the bowels are remarkably torpid, and the alvine discharges characteristically unnatural. In pseudo-hydrocephalic cases, experience has fully established the advantage of purgatives, and of medicines calculated to excite more natural secretions from the liver, and other chylopoetic organs. Cases, too, of hydrocephalus, or cases, at least, so similar in all the symptoms to those which characterize fatal and undoubted cases of the disease, as to have been deemed truly hydrocephalic by the most judicious and experienced physicians, have now and then yielded to, and terminated favourably by, a similar mode of treatment. The authority of Dr Hamilton, the author of the Observations on the Utility and Administration of Purgative Medicines, will, it is presumed, be generally admitted as the best evidence of the truth of this important fact. Dr Hamilton's work, the first edition of which was published in 1805, was soon followed by the Second Part of Mr Abernethy's Surgical Observations. Both these publications contain many important observations, which demonstrate the great advantage to be obtained in the treatment of many diseases, by remedies directed to the improvement of the functions of digestion and chylification; and some of their cases shew, that even hydrocephalus acutus comes within the sphere of this practice, as indeed do a large proportion of the diseases of childhood.

The influence excited by the liver on the abdominal functions, and, through these, on the general system, and the indications afforded by the sensible qualities of its altered secretion, have also become familiar to physicians, more especially, perhaps, since the publication of Dr Sanders's work on the liver. Nor can we here omit mentioning the name of Dr Curry, whose views and opinions, though yet unpublished, have had their share in directing the attention of others to this field of observation.

But it is to one of the authors now before us, Dr Cheyne, that we are indebted for the first distinctly pronounced opinion, that the origin of hydrocephalus is, in many instances, to be found in the abdominal viscera. Dr Cheyne published his first Essay on Hydrocephalus at Edinburgh, in the year 1808; a work which has been already noticed in the fourth volume of this Journal. He remarked that children under hydrocephalus early in the disease, when the region of the liver had been handled, complained much more than when the same pressure was applied to any other part of the abdomen; and upon dissection of hydrocephalic children, that he found in the liver the remains of great inflammatory action, and also proofs that undue irritation had existed in the alimentary canal. In his second Essay, now before us, he again observes,

> "In a great proportion of the cases of acute hydrocephalus, which have fallen under my care, before any affection of the brain could be perceived, there were various symptoms of disorder, either of the secerning part of the liver, or of the mucous surface of the stomach and intestines; and, in many of these cases, the hypochondria, in the early part of the disease, were tumid, the liver was enlarged, sometimes it was the seat of pain, at other times great uneasiness was occasioned when moderate pressure was applied to it, through the integuments of the abdomen.
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> "In many of the dissections which have been made after hydrocephalus, the liver has been found with the stamp of inflammation; it has been found enlarged, tied to the peritonæum by preternatural adhesions, studded with tubercles, and otherwise deviating from its sound state; vivid remains of inflammation in the intestinal canal have also appeared, and sometimes those constrictions and volvuli which are the effect of irritation and spasm in the primæ viæ; lastly,

there has been observed considerable enlargement of many of the mesenteric glands."

"In many instances, a combination of symptoms which might fairly be imputed to the formation of hydrocephalus, has yielded to a timely exhibition of cholagogues, which, while they improved the character of the fecal discharge, reduced the volume of the liver, and restored the functions of the stomach.

"Such were some of the considerations which led me to conjecture that the affection of the brain, in hydrocephalus, is often secondary: that, in most cases, it is owing to irritation in the abdominal viscera, and especially in the liver. This doctrine formed one of the principal features of an essay on hydrocephalus, which I published in 180

"I have now the satisfaction of knowing that several professional gentlemen of eminence, both in London and Dublin, have long entertained nearly the same opinion. It is twelve years since Dr Curry of Guy's Hospital, in his lectures on the practice of Physic, began to announce an ingenious pathological theory, with respect to the influence exerted by certain diseased states of the liver on the whole constitution, or on some individual organ. From a general view of hydrocephalus, and a comparison of it with other diseases of the brain, he was led to the conclusion, supported by the most effectual means of cure, that the affection of the brain, though the immediate cause of death, when the case ends fatally, is, in general, only a consequence of inflammatory irritation, with diminished or altered function of the liver. Mr Thomson of Sloane Street, found that evident marks of inflammatory action existed in the liver, in nine dissections of hydrocephalus out of eleven which he had made. In one of the remaining dissections, there was intus-susception of the jejunum, in the other marks of inflammation through the greater part of the colon. And he asserts, that in a majority of the cases of the disease, the organs of digestion are in fault before the head appears in any degree affected; and he adds, that in every opportunity which he had of observing the earliest approaches of hydrocephalus, the bowels have been first irregular, the stomach acescent, and the stools, whether procured by medicine or not, have been fetid or clay-coloured, displaying defective action of the liver, and an imperfect formation of the bile. To these observations he has added a faithful account of the manner in which the disorder is transferred from the abdomen to the brain in those who are predisposed to hydrocephalus. Lastly, many years ago, Dr Boyton, the learned professor of the Institutes of Medicine in the school of physic, was also led to think that hydrocephalus originated in the abdominal viscera, from finding that cathartics were the only means of relief. In the course of a long practice, he has noted fifteen or sixteen cases of hydrocephalus, nascent or confirmed which were relieved by purgative medicines, namely mercurials combined with antimonials and the common purgatives.

Our own experience enables us to appreciate the value of these pathological observations. We have indeed, for a considerable time past, been of opinion that very many cases of hydrocephalus acutus have their origin in the chylopoetic viscera. The precursory symptoms of the disease; the symptoms common to hydrocephalus, and to other diseases of childhood of acknowledged dependence or a disordered condition of the functions of the abdominal viscera; the peculiar appearance of the stools in hydrocephalus; the proofs of disordered action and secretion observed throughout the whole course of the disease; the effects of medicines, and especially of cathartics; and the appearances observed on dissection, form altogether a series of connected observations, quite sufficient to justify the pathology to which Dr Cheyne has called the attention of the profession. A child died after suffering all the symptoms which distinguish the disease called hydrocephalus acutus,—no water was found within the brain,—and the only diseased appearance which Mr Abernethy, an experienced surgeon and anatomist, could detect, was in the bowels. This is not a singular fact. It has been observed again and again. Dr Carmichael Smyth himself could find no water within the brain of one of his hydrencephalic patients; but he had the complacency to persuade himself that it had once been there, and that it had been absorbed previous to the termination of the disease.

We shall pay all proper attention to Dr C. Smyth and his treatise on Hydrencephalus in its proper place; but, before quitting this part of our subject, we shall take the liberty of making use of another of his cases, not certainly to illustrate any position or doctrine of his own, but as a farther proof of that connection between abdominal disease and hydrocephalus, which *a Dr Cheyne of Edinburgh* (now, however, a physician and professor of Dublin) has endeavoured to make known. This case bears for title, " a Case of Hydrencephalus, with distention and inflammation of the bowels, with the appearances after death, by Dr J. C. Smyth."

" M. S. eleven years of age, formerly healthy, well formed and active, was, some days previous to my seeing her, seized with vomiting and purging, the usual symptoms of cholera; accompanied with headach, for which she had taken an emetic and some opening medicine; the purging ceased in three or four days, and on the 13th of March, when I first saw her, she had been three days without any motion. I found her in great agony, from pain in her bowels and head; the whole abdomen, but particularly the præcordia so tense and swelled, that she could not bear the slightest pressure on the part, and at times she beat her forehead with her hand. Her pulse was irregular, and rather slower than natural for a person of that age. The heat of her skin and hands was temperate, and her countenance, though at times distracted with pain, was not flushed; her tongue was white and moist. Apprehending that the *chief* and most immediate danger was from peritoneal inflammation, *which* had either taken place, or might soon be expected—I ordered blood *to* be immediately taken from the arm, the abdomen to be fomented, *glysters* thrown up, and an opening medicine to be given every four hours, until proper evacuations should be procured.

This treatment so far succeeded, as to relieve the pain and tenderness of the abdomen: but she had no sleep, and next morning I still found her screaming as before, but chiefly from the violence of the headach, complaining that her head was splitting asunder. Upon examining her eyes, I found the pupils considerably dilated; which circumstance, with the violence and unremitting acuteness of the headach, led me now to suspect some material injury to the brain, and that the headach was not altogether symptomatic or secondary.

"On the 20th her countenance continued to be rather pale, the heat of the body temperate, the pulse irregular, and unless when she was agitated by the violence of pain, rather slow.

"On the 21st, the enlargement of the pupils, and insensibility of the eyes to light, increased; one of the eyes appeared *blood-shot*, and the strasbismus or squinting was now shocking to *behold*.

"On the 22d the headach continued with almost unremitting violence, she never slept, was said to be light-headed in the night; but I found her always perfectly sensible during the day, and her senses and speech remained until within five minutes of her death, which happened on the 23d. About twelve hours before her death, her pulse became quick, and her mouth rather parched, with a rattling in her throat; when asked where her pain was, she put her hand on her forehead, then on the præcordia.

"During the few days that I attended her, she was *freely and repeatedly* purged, by means of calomel and scammony, with some of the neutral salts. Her bowels were constantly fomented, and glysters thrown up; besides which, she took small doses of calomel with camphor in powder, washing them down with a saline mixture and vitriolic æther; blisters were applied to her head, back, and behind her ears; and on the 22d (the day before her death) I gave her the compound powder of Ipecac. with calomel, which sweated her; and at first she seemed relieved, but the relief was of short duration; she made water freely, and passed some hard waxy feces.

Dissection.

"This was performed the day after her death, by the surgeon who attended her. Upon removing the integuments of the head and cranium, a slight adhesion was found between the dura and pia mater, immediately under the vertex. The veins of the brain seemed *to me* unusually turged with black blood, though the dissector did not think that there was in this any unusual or preternatural appearance. There was a small quantity of fluid between the pia mater and convolutions of the brain; upon cutting through the two hemispheres, to get at the lateral ventricles, blood oozed out from the cut surface of the medullary part, which seemed dotted with a great many red points. Upon removing the cut portions of the hemispheres, the ventricles were seen distended with fluid, the fluctuation of which was very perceptible, and upon cutting into them, they were found to contain a pure pellucid lymph, nearly two ounces of which we were able to collect in a tea-cup; and making allowance for what was spilt and found mixt with blood at the bottom of the scull, the whole quantity could not be estimated at less than three ounces. Water was found upon the sella turcica; and the cellular membrane surrounding the optic nerves had an anasarcous appearance.

" *Abdomen.*—Upon opening the abdomen, the colon presented itself greatly enlarged and distended, and upon examination, was found to be in this state from the caput coli, for the whole of the ascending and transverse portions of this bowel. On the left side, immediately under the curvature, it suddenly contracted, and to a degree I never saw before, being much smaller than any of the small intestines; and continued so all the way into the pelvis. In several places it appeared to have been inflamed, and upon laying it open, the villous coat was found extremely red, and covered with inspissated mucus. The small intestines seemed to have been greatly inflamed, and the ileum, at one part, was of a colour approaching to gangrene. The appendix vermiformis was five inches in length, and adhered to the posterior part of the colon.

" The stomach was out of its natural situation, drawn upwards to the left side, small and contracted, especially towards its lower and great end, and upon gently pulling it, it burst near the cardia; upon laying open the stomach, an ulcer or abscess was found to have been in the part where it burst; the coats of the stomach, at this place, were all (the peritoneal excepted) entirely destroyed, nothing remaining but a greenish mucus."

This case is really an interesting one; but after what has been already said, any lengthened commentary were superfluous. We shall just remark, that it presents us with an undoubted example of a disease beginning in the abdominal viscera; confined to those viscera for some days; then attended with the regular stages and symptoms of hydrocephalus; at length terminating fatally; and lastly, exhibiting to the eye of the dissector, plain and unequivocal traces of disorganization, both in the abdominal and cranial viscera.

Cathartics are remedies seldom omitted in the treatment of hydrocephalus; and calomel, we believe, is as constantly administered throughout the course of this disease. But cathartics have generally been prescribed occasionally only as mere evacuants, and with the view of obviating costiveness; and calomel has been administered empirically, or as a cathartic only, or as a remedy supposed capable of promoting the absorption of the dropsical effusion within the brain.

Dr Cheyne, however, employs these remedies with a view to change the actions of the secerning vessels, and more especially to encourage and improve the biliary secretion. Hence his attention to the bowels is steady and persevering, and he varies his means, and regulates his remedies, according to the nature and degree of the visceral derangement and obstruction.

"The state of the hypochondria, the nature of the stools, and the other excretions, the appearance of the tongue, and the smell of the breath, ought to be examined with care. If the patient wince when the right hypochondrium is pressed, leeches ought to be applied to it, or the margin of the ribs may be cupped and scarified; if there be much pyrexia with headach, blood must be drawn from a vein, or from the temporal artery. Then cathartics are to be given to promote, and, if necessary, to alter the secretions; generally calomel, with small doses of some common purge of an active kind, as rhubarb, jalap, or scammony; and, if there be a sickly smell of the breath, and fulness and uneasiness at the pit of the stomach, an *irritation* of the mucous membrane of the intestines is denoted, which is sometimes relieved by mild antimonials, these, consequently, are to be added to the cathartics; squill may also be exhibited with the same view, more especially when the urine is deficient. If the stools be dark, green, and glairy, most probably the common cathartics will have little effect; indeed we cannot expect that they will change the nature or appearance of the secretion which issues from an organ over which they have little control."

Even calomel will fail in accomplishing this, till the irritation and increased action of the liver has been moderated or subdued. If the viscera, he says, and particularly the liver, are in a state of high irritation, this irritation ought to be allayed before the stimulus, which increases their secretions, can be employed with advantage, or even with safety. "The true practice is, in the first place, to reduce arterial action by venesection, or by topical bleeding, and then to restore the secerning functions of the viscera by means of calomel and other cathartics."

Where bloodletting is contraindicated by the diathesis of the patient, and yet it is desirable to moderate the increased action of the vascular system, Dr Cheyne is led to make use of antimonials. James's powder has been said to have been successful in a number of cases of hydrocephalus, in which it had been empirically employed in Dublin. With Dr Cheyne it has failed; but antimonials, in combination with cathartics, have appeared to him very useful in infantile remittent, and other cases liable to degenerate into hydrocephalus. If, however, "after due preparation, by means of venesection, or topical bleeding and blistering, calomel and James's powder in combination, or alternated with the common purges, should fail of producing a change in the appearance of the stools, or of giving relief to the head, other measures," he observes, "ought to be tried." Under these circumstances of failure in effecting a favourable change, accident, Dr Cheyne tells us, led him to what appears to him a considerable practical improvement. In a case where recovery was despaired of, an opiate was given to allay pain and irritation, five grains of pulvis ipecac. comp. with a like quantity of hydrargyrus cum creta; and, in an hour or two after taking this medicine, the character of the pulse was improved, the patient enjoyed a respite from pain, and the stools became more feculent. He passed through the third stage without suffering, by keeping him under the influence of opium, and his life seemed prolonged rather than shortened by it.

A series of cases is afterwards related by Dr Cheyne, in which opium was given; in which, after depletion, and in combination with mercurials, or mercurials and antimonials, that medicine appears to considerable advantage. The practice, however of administering opium in hydrocephalus, we must inform Dr Cheyne, is not new: It was employed by Dr Percival; but, as we have not his book before us, we shall give the practice from Dr Underwood.

"The intentions of cure, as laid down by Dr Percival, are, to mitigate the pain and spasm, to promote absorption, and to increase the serous excretions. When the pain and spasm are considerable, he advises opiates in large and repeated doses, unless there be coma; and, in that case, he substitutes musk combined with sal. cornu cervi. He directs repeated blisters to the head, and recommends joining the digitalis with opium and calomel, though he is inclined to think that the good effects of this composition are rather attributable to the combination of the opium and calomel than to the digitalis. If the mercury be disposed to run off by the intestines, he guards it farther by opium."

The pathology which thus traces back the origin of hydrocephalus to disordered actions previously begun in the chylopoetic organs, is of peculiar interest, in a prophylactic point of view. When the excitement has been extended to the brain, and the hydrocephalic actions have been organically established, our best directed endeavours will still too often disappoint our hopes of preventing the disease from running on to a fatal termination. By a jealous attention, however, to the earlier symptoms, to those which have been called precursory, and which are always dependent on some disorder of the abdominal viscera, and by a prompt, persevering, and judicious employment of remedies of unquestionable efficacy, we may reasonably and *confidently hope* to arrest the progress of disease, and prevent the *succession* of those trains of symptoms from which so few have hitherto recovered.

Dr Yeats has accordingly, in his admirable letter to Dr Wall, invoked the attention of the public to the early symptoms which lead to the disease termed water in the brain. He has pointed out the necessity of a watchful attention to those symptoms, and the fatal consequences of neglecting them. These early symptoms, which, when understood, may be so easily controlled, but which, when neglected, misunderstood, or overlooked, so often lead to hydrocephalus, have not their origin in the brain, but in the abdominal viscera; and, with the first train of symptoms which form the ground-work of future disease of water in the brain, our practice, it is justly observed by Dr Yeats, must commence. "If we proceed," he says, "upon the principle that this is a disease of *the head only*, we shall be exposed to constant disappointment in our practical expectations, *and to* unavailing regret for the commission of errors.

"In the very first commencement of the symptoms, before any alarm is taken, and before any person can possibly imagine, but from experience, that they will often terminate in water in the brain, an occasional languor, as if arising from fatigue, with intervals of considerable activity, is observed; it is therefore attributed to this cause. from the disposition, too, which the child manifests, of reclining on the sofa, chair, or lap of the mother; the usual degree of healthy appearance of the countenance diminishes, though not permanently, is a transient paleness and occasional collapse of the features; a dark coloured line is observable under each eye, with a dulness of that organ; the usual softness and pliability of the skin diminish, with a consequent harshness and increased heat on the surface; the appetite capricious; occasional thirst; state of the bowels more than commonly tardy; the tongue white, and rather disposed to be dry if examined in the morning; the pulse at this period exhibits no particular morbid change, either in its frequency, strength, or regularity; the urine is at times higher coloured than it ought to be, and from observing that the child has not had an evacuation by the bowels as often as usual, recourse is had to some domestic purgative, and a stool is procured more than commonly consistent and firm, and not in the same quantity as formerly with the same dose of the medicine; no very striking alteration of colour is observable, unless attentively examined, when it will plainly appear, that a diseased secretion has already begun to take place in those glands, which pour their secretions into the intestinal canal; at times the evacuation will be throughout, much lighter than it ought to be; at others, only partially so, and again the whole will be more tinged with a darker colour of a greenish cast, and accompanied with some quantity of slimy matter: but more than the mere abrasion of the intestines by a purgative will produce. When any uneasiness in the head is complained of, it is not of pain either acute or dull, but of a disagreeable noise and confusion; the scalp, at times, feels sore on being rubbed or touched.

"During this state, upon examination, a puffiness will be felt, and also a fulness observable over the centre of the stomach, extending towards the navel; uneasiness is complained of there from pressure, but, like all the other symptoms at this time, they are not permanent; and the only symptom which observes any permanency, is the torpid state of the bowels, although the degree of it varies in different patients; the costiveness is, nevertheless, always more or less present; the sleep is frequently disturbed by restlessness, indicated by repeated movements about the bed. The child is said to be only not well, and this is supposed to arise from some improper food that has been taken. It is evident we cannot, *a priori*, positively determine what exact state of disease this deviation from general health will ultimately produce; but full well I know, that this irregular excitement, this vaccillating state, in the way above described, very frequently leads to the next chain of more manifest morbid actions, which terminate in water in the brain. We should be, under such circumstances, most carefully watchful."

Every experienced practitioner will acknowledge the correctness of this description; and we, too, full well know, "that this irregular excitement, this vaccillating state, very frequently leads to the next chain of more manifest morbid actions which terminate in water in the brain." It is this state of health, however, so full of danger if neglected, which can so easily be corrected by a seasonable interference of art, and which generally yields to a course of cathartic and alterative medicines. Nothing very alarming indeed has yet occurred, but it is better and easier to obviate the occurrence of alarming symptoms, than to remove them. "I would much rather," says our intelligent and candid author, "incur the charge of having twenty times supposed cases of water in the brain, by attacking what I consider to be the early symptoms, than once ultimately become a witness to this distressing scene, by neglect, oversight, or mistake.

"If, unfortunately, this should be the case, the symptoms assume a more formidable and commanding shape: the occasional languor wears more the appearance of permanent lassitude; the returns of activity diminish; the child wishes to be almost constantly in a recumbent posture; the unhealthy look of the countenance becomes more permanent, and more observable in every respect; the darkness under the eyes is of a deeper colour; the excitement from feverish action becomes more regular and more apparent, with the consequent harshness of the skin: occasional flushes pass across the cheeks, sometimes more fixed in one cheek; transient pains are felt in the head, more or less acute, and more or less frequent, and at times when the child will be apparently enjoying itself with comfortable feelings, its attention will be suddenly arrested by this pain, crying out, 'Oh my head aches!' Some will complain of the head feeling sore to the touch externally. The pulse now becomes at times much quickened, not particularly irregular; but if carefully examined, and it must be done with some attention, when the child is under the febrile accession, an irregularity will be readily discovered, once, twice, and sometimes more in the minute. Periods of drowsiness supervene; the bowels are more obstinately torpid, and when stools are procured they are of a very disagreeable smell, and of a very morbid appearance; sometimes a glutinous mass intermixed with dark lumps of feces, at others there is a mixture of deep green, with matters similar to yeasty fermentation; their colour and appearance will vary much in the same person at different times. Sickness, nausea, and vomiting are frequently troublesome, either when the little patient raises his head from the pillow, to which drowsiness and lassitude had consigned it, or after taking food, or both. In some, the puffiness and fulness about the region of the stomach, are not now so perceptible, one part of the morbid actions having yielded to others of a more violent nature; this symptom, though common, does not invariably attend; its being observed, too, depends upon what portion of the digestive organs are most under morbid action at the time of examination. All the symptoms bear evident marks of irregular excitement: a giddiness, with an unpleasant cloudiness in the sight, is complained of, and although the eyes exhibit nothing morbid upon examination, a strong light is disagreeable and painful; the urine varies much in colour and quantity, depending entirely on the circumstance of the febrile accessions; the appetite becomes deficient; the thirst troublesome; the tongue white and inclining to be dry. The complaint in this stage of its progress is still manageable, in some easily so, from the circumstances of the previous habits of the child, as already stated; but it must be recollected, that every hour is now most precious, and any moments lost are scarcely to be recovered; for, in proportion as the symptoms form more a disease of the head, so is it the more dangerous, and consequently with the greater difficulty removed.

"If it should so happen, that an idea is entertained that this assemblage of symptoms arises from foul bowels, without having in view their ultimate consequences in producing a disease of the brain, unfortunate, indeed, will it prove; too late will the error be seen, and frequently fruitless the attempt to correct it. I very greatly fear that mischief has been done by the loss of much precious time, in making too nice a discrimination between the symptoms which lead to water in the brain and the infantile remitting fever, on which Dr Butter has written so well. As it often happens, too, that a worm is passed at this time, it serves to confirm the opinion entertained of the cause, and the fatal delusion decides and continues the inefficacious practice adopted."

Vascular excitement, it is observed, has now taken place; and, according to the force of this, it becomes necessary to take away blood, generally and topically. The detraction of blood our author thinks indispensable in one way or other; most frequently the local depletion is called for, and then the powerful alterative effects of mercury are to be called into aid. If the evident inflammatory tension of the head be allowed to continue, fatal effusion into the brain will be hastened and increased; and, from the preternatural vascular activity which also exists in the organs subservient to digestion (the morbid action of which produces those very badly coloured stools) "the application of leeches to the region of the stomach and liver, as well as to the head," he is of opinion, "will be found essentially useful." For the same reason, he advises the neutral purging salts, particularly sulphate of potass, to be given twice or thrice a-day, which he has from experience found to answer better than resinous purgatives, which are too heating at this period of the disease. "But we must never fail," he adds, "to give two or three grains of calomel every night." Blisters he thinks doubtfully of; but he affirms that full evacuations should always be premised. The diet should correspond with this antiphlogistic treatment,—animal food, spices, and wine should be avoided. But when, after pursuing this plan for a due time, and the morbid symptoms have in consequence declined, some light tonic, especially if combined with an opening medicine, sufficient to insure a daily evacuation of the bowels, will be useful; such as a few grains of columbo or cascarilla, with two or three grains of rhubarb or aloes, twice a-day.

"Upon a steady perseverance in, and a judicious application of the remedies to the degree of violence and variation of the symptoms during the whole of this period, will depend the safety of the patient; by the prevention of the progress, sometimes very rapid, to the next stage of their increased activity, danger, and too frequently fatal consequences. The accession of this state is marked with greatly increased violence, and with great suffering to the patient: the heat of the skin becomes more intense and harsh; febrile accessions more violent and distressing; the pains of the head more acute, and more frequent in their return, and the loud screams of the child on this account are truly afflicting; the pupils of the eyes shew great dilatation, but still contract on the approach of light, though not healthily, by a waving languid vibratory motion; a squinting takes place at times; double vision is complained of, and when the child is desired, though not seeing double at the time, to view an object, I have noticed that he sees the object, not where it

> really is, but on one side of it, by pointing to the spot; a knitting of the eye-brows, with an expression of countenance indicative of great distress; for a few minutes there will be a perfect silence and quietism, with a fixed steady stare of the eyes, and a very great dilatation of the pupils, when a sudden start will take place, with a loud screaming and a quick tossing of the arms over the head; frequent moaning; deep sighing; sickness and vomiting; bowels most obstinately costive; the evacuations when procured are very scanty and ill formed, and extremely offensive; and when it happens that by any active means a good mass is brought away, it looks like any thing but feces, being dark, yeasty, and gelatinous, smelling like a mixture of sour grains with putrid matter; the tongue foul, sometimes brown and dry; much thirst; no appetite; the urine irregularly secreted, both in colour and quantity; the pulse is very irregular, both in the tone of the vibration and in the flow of the blood; sometimes slow, sometimes quick and intermitting, with a tensive feel, until it at last sinks into permanent sluggishness, ushering in its ultimate and fatal celerity; a dewy moisture settles in drops upon the upper lip and around the nose; a considerable wasting of the flesh has taken place; the countenance pallid and sunk, with a hollowness of the temples; blueness of the lips, with their frequent retraction from an attempt but inability to cry, ending in a whining tone from weakness; the eye-lids half open and motionless; the eyes filmy and fixed with a peculiar stare from the extreme dilatation of the pupils; the circulation is extremely hurried; convulsions frequently take place; palsy supervenes, either partially or generally, and death, most commonly in one convulsive struggle, closes the painful scene."

Even in this state, so full of danger, and so often fatal, Dr Yeats does not despair. The serous effusion within the brain is the consequence of excitement, or increased vascular action; and this again of the previous excitement, and still existent disorder of the digestive functions. To diminish excitement, we must have recourse to topical bleedings, to blistering, and to the administration of mercury, both externally and internally. These, with a watchful attention to see that the bowels are daily emptied, are the means resorted to, and sometimes with success.

> "And although there shall be every appearance that effusion has taken place, the patient not unfrequently completely recovers, by the use of those medicines which carry off effused fluids, more especially when the constitution is under the influence of mercurial action."

We think Dr Yeats has here expressed himself too incautiously. We wish that, instead of *not unfrequently*, he had said *sometimes*, recovers; and this correction we advise our readers to consider as nearer the truth.

Thus far we have endeavoured to bring our readers acquainted with the very interesting observations with which Dr Yeats's publication abounds. Many remarks of practical importance we have necessarily passed over; and our limits forbid us to follow our author through those pages in which he endeavours, more generally, to trace the connection of morbid actions, and to determine that more particularly which exists between the chylopoetic viscera and the brain.

To meet Dr Smyth we must make a retrograde movement. For, although the treatise on hydrencephalus now before us appears to have issued from the press in the year of our Lord 1814, we are informed that it was written upwards of twenty years ago, at the request of a nobleman, whose desire was to his physician a command, and a command the Doctor was proud to obey. Retirement, too, from public employment, and unforeseen and unfortunate events, appear to have concealed from our author, what, in the course of the last eleven years at least, had appeared on the subject. After his manuscript, however, had been sent to his bookseller, he received an Essay on Hydrocephalus Acutus, published by a Dr Cheyne of Edinburgh, in the year 1808. From this Essay he found that opinions were abroad directly opposite to his own, both in respect to the nature of the complaint, and likewise the proper method of treating it. Some of these opinions seem to have surprised and shocked Dr Smyth in no small degree. Dr Cheyne, he tells us, professing to illustrate still farther the hydrocephalus, or dropsy of the brain, attempts to prove that no such disease exists.

> "For," says Dr Smyth, "if the effusion of a watery fluid into the ventricles of the brain is not the original disease, and cause of the symptoms, but an accidental occurrence, the consequence of some other morbid affection of the brain; and if, even when it does take place, it does not occasion any symptoms, or interruption of the functions of life, it cannot be reckoned a disease, wanting the essential character of such."

This, to be sure, is somewhat alarming. But when we affirm that the mucous discharge in catarrh, is not a flux from the brain,—that it is not the cause of the disease called catarrh, but the effect of a previous excitement, and increased vascular action of the membranes of the nose, fauces, and bronchia; nay, even when we assert that the disease called catarrh may exist without any increased mucous discharge, we are far from attempting to prove that no such disease as that called Catarrhus acutus exists.

When, indeed, Dr Cheyne speaks of hydrocephalus as a diseased action of a particular kind,—when he calls this action specific or hydrocephalic—we agree with Dr Smyth, that this is no explanation whatever of the nature of the disease. But neither do we think Dr Cheyne deceived himself so far as to view this in the light of an explanation. It appears to us that Dr Cheyne meant rather to say what hydrocephalus is not, than what *it* is, by this mode of expression. According to him, it is not a mere dropsy of the brain, neither is the excitement, which precedes the watery effusion, ordinary inflammation. The different modes and varieties of inflammation, it is observed, are not sufficiently ascertained. The excitement on which hydrocephalus depends, is a diseased action of a particular kind; a term which he employs negatively, just as he would say syphilis is a diseased action of a particular kind.

"The hydrencephalus," Dr Smyth tells us, "is a species of encysted dropsy, where the fluid is lodged in the very centre of the brain, immediately under the corpus callosum, occupying chiefly what are called the lateral ventricles." As he does not consider this disease to differ in any other particular, from other cases of dropsy, than in the peculiarity of its situation, he very methodically opens his investigation with some general remarks on the nature and causes of the genus. There are but two causes of dropsy admitted by our author, obstruction to the return of venous blood to the heart, and debility. Now, as Dr Smyth has been so much surprised at some of the opinions which have got abroad respecting the nature of hydrocephalus, we think he would be no less astonished, were he made acquainted with some of the changes of opinion which have taken place respecting the nature and causes of dropsies in general, and especially were he told, that the doctrine of debility has become now so very unfashionable, that physicians are actually employed in shedding the blood of those debilitated patients who are labouring under typhus, diabetes, and dropsy; and yet it appears, from the late return made to parliament, that the population of the country is increasing, and mortality diminishing. "My notion of a dropsy," says Dr Cheyne, "is nearly conveyed in this aphorism of the venerable Heberden: Hydrops non tam ipse morbus est quam alicujus morbi signum." It is obvious, that, before the dropsical effusion takes place, the condition of the part, as it exists in health, must be altered, and this antecedent condition of the part is the disease. "*Accelerated circulation as certainly precedes the effusion of lymph, unless* arising from a strangulation or rupture of a lymphatic, *as it does the formation of purulent matter.*" And Dr Yeats, after speaking of this excitement, and increased vascular action which precede dropsical effusion, concludes by observing,

"The opinions which prevailed, that the original cause of dropsy was always debility, gave rise to a uniform mode of practice, not always consistent with the symptoms. A doctrine is gaining ground, and we may look to it with advantage by practical application, *that bleeding is beneficial in some species of general dropsy, and which may be termed inflammatory, from evident marks of vascular activity.* I have experienced decided advantage from *bleeding, and the antiphlogistic treatment in the dropsies to which I allude.*"

For a full and satisfactory explanation, however, of this doctrine and practice, we would refer our readers to Dr Blackall's publication on Dropsy, a work which has added more to our knowledge of the diseases of this genus, than the writings of any preceding author, ancient or modern, have done.

But, according to Dr Smyth, hydrencephalus is not only a pure dropsy of the brain, but a dropsy arising from debility; and the sole cause of all the symptoms which make up the disease in question. And it is his decided opinion, that not one symptom of the disease can exist before the dropsical effusion have taken place. He makes indeed a distinction between primary and secondary hydrocephalus. By the former he means that which has been preceded in the attack by no other disease; and by the latter, that, the attack of which supervenes to other diseases, which according to his observation, are two only, scarlatina and infantile remittent. But he does not consider these preceding diseases, or any other indisposition, as forming any part of the dropsy of the brain, or being otherwise related to it, than as giving a predisposition to the disease; so that the attack of what he calls secondary hydrocephalus is as definite in its commencement and course as the primary, or that which has not been preceded by any other complaint. This was necessary, in order to get rid of all those symptoms of indisposition which have been called precursory, and which often precede the formal attack of hydrencephalus for weeks, or a much longer period. Accordingly he rejects the duration to which, from this observed fact, Dr Whytt had been led to extend the disease.

The most curious part of Dr Smyth's book, is that which he has chosen to entitle Diagnosis; but which better merits the title of a theory, or ratio symptomatum.

"Although," he observes, "the symptoms of hydrencephalus have only one origin, viz. the collection of a watery fluid in the ventricles of the brain, yet this cause, simple as it is, produces two distinct and opposite effects; and consequently the disease is accompanied by symptoms of a kind and character, not only different, but directly opposite to each other. For while the weight and pressure of a watery fluid in the centre of the brain causes symptoms of compression, or of a compressed brain, the distention of the delicate membranes lining its ventricles, produces symptoms of high *irritation*; and though, in the course of the disease, sometimes the one set of symptoms, and sometimes the other, predominate, or take the ascendancy; from first to last, this strange admixture of opposite symptoms is perfectly evident."

Now if we understand Dr Smyth rightly, it is this strange admixture of opposite symptoms that constitutes the certain diagnosis of hydrocephalus, or which is the sign of the existence of water within the brain, and of no other disease.

The symptoms of irritation are said to be, the symptomatic fever,—headach,—pains in the nape of the neck and limbs,—morbid sensibility to light and sound,—squinting, perhaps dilatation of the pupil,—irritability of temper,—sickness and vomiting,—screaming,—catchings and twitchings,—convulsions.

The symptoms of compression again are,—*slow intermitting pulse*;—interrupted breathing, and deep sighing;—*dilatation, and imperfect contraction of pupils upon the admission of light*;—*their subsequent paralysis, and that of the muscles raising the eye-lids*;—*the insensibility of the retina, and total loss of sight*;—costivenesss and insensibility of the bowels to purgatives,—*of the skin to blisters,—and, in short, of the whole system* to the usual *stimuli*;—*urine and stools coming away without consciousness*;—*loss of the power of swallowing*;—a constant drowsiness or dozing, *terminating in coma*. These, he adds, are the well known and usual symptoms of compressed brain, which, combined with those of irritation, give from the beginning, and during the whole course of the disease, a peculiarity of character that no other possesses.

As examples of the application of this very curious diagnostic sign of water in the brain, take the following: "Dr Whytt has mentioned the morbid sensibility of the eyes to light, to which symptom may be added, a slight degree of squinting, both symptoms of irritation, which are always accompanied more or less by the dilatation and imperfect contraction of the pupils, well known symptoms of compression." Again, irritability of the stomach, with torpor of the bowels,—a slow and irregular breathing, with a quick and regular pulse. In a word, this concurrence of two classes of symptoms, arbitrarily assumed, as those of irritation and of compression, constitute an infallible diagnostic, or sign of the existence of water within the brain, and of no other disease. "I am perfectly ready to allow," he does indeed observe, "that many of the symptoms of irritation are caused equally by teeth, worms, and other irritating matters affecting the alimentary canal; but I do affirm, that no irritation occasions the constant combination of opposite symptoms above described, *besides many* other circumstances peculiar to and characteristic of this disease." Now, be it recollected, that this diagnosis is intended to enable us to distinguish the disease in what has been called its earlier or first stage, that is, when but few of those symptoms which Dr Smyth has classed as those of compression are observable. In the enumeration which we have given, from Dr Smyth's book, of this class of symptoms, we have desired those which do not generally occur till the advanced stages of the disease, to be printed in *italics*, so that the reader has only to glance over it, in order to perceive what we insist upon, that few or none of those symptoms are observable during the first stage of hydrocephalus, and that the concurrence of Dr Smyth's two classes of opposite symptoms cannot serve as a diagnostic of the disease at this early period; and, at any subsequent period, Dr Smyth observes, " a knowledge of the disease, however useful for the credit of the physician, can be of little advantage to the unhappy patient." Besides, not only do many of those symptoms, which our author calls symptoms of irritation, occur in teething, worms, and fevers from foul bowels, but many also, *we affirm*, of those which he has classed as symptoms of compression. " But in all those cases," he observes, " and others, which it is unnecessary here to mention, the heat of the mouth, with the swelling and redness of the gums, are sufficient evidence of the cause." After all this mighty discovery, then, these signs of local irritation, which shew the cause of the symptoms to originate in the teeth, in the bowels, or in the head, are the ground-work of Dr Smyth's *practical* diagnosis at least; yet he has attained such perfection in his diagnostics, " that he can declare with truth, that he never met with a case that gave him the smallest doubt."

The watery fluid, distending and irritating the membrane of the ventricles, and compressing the substance of the brain, affords to Dr Smyth a ready explanation of all the symptoms of hydrocephalus acutus. " There is one symptom, however," he observes, " the enlargement and distention of the veins of the head, and puffy swelling above the nose, which, having escaped the observation of Drs Whytt and Fothergill, and *belonging to neither set of symptoms described*, remains to be accounted for." What more easy ? The explanation appears to our author simple and natural. " The fluid collected within the ventricles must, by its pressure on the longitudinal sinus, and other internal veins of the head, prove an impediment to the free return of venous blood from the brain, and, of course, occasion a greater proportion of that fluid to be returned by the external veins, which necessarily cause a swelling or distention of them." To us, however, this explanation appears most clumsy and unnatural. It is not true, in the first place, that the longitudinal sinus, and other veins in the head, are compressed, for we find *them*, on dissection, turgid and loaded with blood; and the thing is impossible, in the second place, for the sinuses of the brain are firmly bound and protected in the unyielding and tense duplications of the dura mater; and that the longitudinal sinus should suffer the slightest compression from any quantity of water within the ventricles of the brain, far less from the small quantity commonly found in hydrocephalus acutus, is something more than we can comprehend. Whether or not the brain itself be compressible, we will not undertake to decide. But we are quite sure that Dr Smyth's mathematical demonstration, that the brain can be, and is compressed, is most vain and fallacious. His demonstration is this: Inscribe a smaller sphere within a larger; the larger sphere will represent the scull-case, originally quite full of brain; the smaller a space formerly occupied also with brain, but now with water. Therefore the *whole* brain, which in health occupied the whole space of the larger sphere, is now compressed and squeezed by the hydrocephalic water, so as to be confined within the smaller space between the two spheres. It therefore follows, that the brain is compressed. Has it never occurred to Dr Smyth, that something may have been displaced or removed to make room for this water contained in the ventricles of the brain?

As to the appearances observed on dissection, our author can discover no other morbid change than the watery effusion; and he confidently asserts, "that neither the brain itself, nor its membranes, *in any case of genuine hydrocephalus that ever yet has been examined*, shewed any appearance of inflammation, or of the usual effects of this having taken place." Such effects, however, are sometimes observed. In Dr Smyth's first case, we have an example of this;—"a slight adhesion was found between the dura and pia mater." Adhesions and effusions of coagulable lymph have been observed by other dissectors. "On the cerebellum, under the tentorium, there was a deposition of coagulable lymph, but not of great extent."—*Cheyne.*

Considering hydrocephalus, then, as a dropsy arising from debility, it is clear that Dr Smyth can have no other indications of cure than to promote the absorption of the dropsical fluid, and to obviate the debility whence it has arisen. Mercury appears to have been at first as unsuccessful in his hands as it has been with others, when entirely confided in; yet he continued to entertain the highest opinion of its efficacy, and thought it might prove more useful if combined with other medicines, by which its effects might be determined to the kidneys, where its operation, at least, would be attended with no risk or inconvenience. He therefore adopted the following formula: "Ten grains of quicksilver was rubbed down with twenty or thirty grains of cordial confection, or manna, to which afterwards five grains of the fresh squill was added, and the whole made up, with syrup, to the consistence of a soft bolus, or electuary, to be divided into such small portions as was necessary for its being administered along with a little gruel, panada, or the like." The dose above mentioned, that is, ten grains of mercury, with five grains of the fresh squill, was given every six or eight hours, "when the child's stomach could bear it."

With this remedy he has recovered four cases, and promoted, he thinks, the absorption of the water in two others, who did not recover, "besides two others more that came under my care, where, though the disease terminated fatally, an absorption of the fluid took place, as *was apparent in the one from the symptoms*, and was confirmed in the other, by the examination of the brain after death."

Another part of the treatment in the hydrencephalus, which he considers as of the first importance, is the application of caustic to the bregma. "The caustic has the advantage of a blister, being more certain in its effects, and more powerful in its operation," &c. The caustic he usually employed was the lunar caustic reduced to a powder, put on the surface of any adhesive plaster, spread on strong leather, of the size and shape of an elongated half-crown, *renewed every twelve hours, until it produces a sufficient eschar, destroying the skin of the part.*

We have only one small objection to this practice in hydrencephalus acutus, however useful such practice may be in scrofulous tumours of the spine and joints, and our objection is this,—that hydrencephalus is an acute disease, which, in most cases, must have attained its second, and perhaps third and fatal stage, before the discharge from the caustic can be established. The ordinary duration of hydrencephalus, Dr Smyth tells us, is about three weeks, or in some cases four. Now, we have had some experience of caustics, and we have commonly found an eschar formed and the skin destroyed in a few hours; but this eschar, and especially the eschar produced from lunar caustic, seldom falls off or separates in less than a week or ten days afterwards; and on the head we have known fourteen days elapse before the eschar was completely cast off, and the issue established. How Dr Smyth or his surgeon managed it, we know not.

But it is clear that our caustics and issues, though they have done good in chronic diseases, would be rather tardy in a disease so acute as hydrencephalus; and that, in most cases of the disease, the patient would have a good and fair chance of carrying the eschar with him to his grave. For this reason we are still disposed to prefer blisters in acute diseases; and though we have often applied them to the head, even in hydrencephalus, we have not known them fail to produce their usual and wished-for effects.

> "The remaining part of the treatment of hydrencephalus relates chiefly to the management of the bowels, and supporting the strength, not only by an appropriate nourishment, but likewise by the use of *cordial and tonic medicines.* An attention to particular symptoms may also in some cases be necessary."

In bringing this long article to a close, we cannot but lament that so little certain is yet known respecting the nature and treatment of this important and most fatal disease. We are quite satisfied, however, that the true field of inquiry is that which has been laid open to us by Drs Cheyne and Yeats.

We had almost forgotten to observe, that Dr Smyth, not satisfied with the term Hydrocephalus, which, to be sure, means no more than dropsy of the head, has substituted for it the juster name of Hydrencephalus, dropsy of the brain;—and that Dr Yeats has adopted this nomenclature.

IV.

A Treatise on the Puerperal Fever, illustrated by Cases, which occurred in Leeds and its vicinity, in the years 1809-1812. By William Hey, Jun. Member of the Royal College of Surgeons in London, and Surgeon of the General Infirmary, and of the House of Recovery, at Leeds. 8vo. pp. 238. London 1815, Longman and Co.

We have been much interested by the perusal of this very able and instructive volume. It exhibits the most demonstrative evidence of the propriety and success of a simple but decided practice, in this severe and dangerous malady, at least in the particular form of it which occurred at Leeds during the periods specified in the title-page. We say in this particular epidemic, perhaps rather in conformity with established opinions upon this subject, than consistently with the impression which the perusal of this work has left upon us. For when we add the testimony of Mr Hey to that of Drs Gordon and Armstrong, and still more when we examine the nature of several of the cases related by the former in this work, we feel disposed to doubt the propriety of the distinctions made by many writers, between the epidemic and sporadic disease.

It appears to us to be an obvious inference from the different views which practitioners and authors now take of many febrile diseases, either that the older physicians of the last generation were mistaken in their apprehensions of putridity and debility, or that the diseases of this country have actually undergone a considerable alteration in their type and character. We are disposed to believe, indeed, that there is some foundation for both these suppositions. We cannot doubt that the hypothesis of debility, and the practice of stimulation, were carried to a much greater length than sound observation would have justified; and that many of the effects ascribed to debility, were in reality the result of over-excitement, not checked in the outset by evacuations, and rather aggravated than diminished, by the cordial system which was afterwards employed. But, on the other hand, there appears to be sufficient evidence of some actual change in the character of febrile diseases since that period, dependent upon other causes than a change of medical treatment, and probably itself in part the cause of this change of medical opinion. It seems to be well ascertained, that petechial fevers have every where in a great measure declined in this country, though thirty or forty years ago they were extremely prevalent in jails, hospitals, and ships, and in the habitations of the poor. At present such fevers are rarely seen, even in the prisons and fever-hospital of London; see Willan on Cutan. Disorders, where he quotes the authority of the surgeon of Newgate, and the physician of the House of Recovery, 2d edit. p. 469; and the experience of practitioners in general will confirm the general fact. Whether these changes are inexplicable upon any known facts, (for we cannot ascribe them with Sydenham to occult changes in the bowels of the earth); or whether they are explicable upon the general improvement of the country in cleanliness, ventilation, temperance, and a better diet, which increasing wealth has introduced, we do not pretend to determine; but as facts we think they must be received. The constitutions of the people seem to have acquired somewhat more of a phlogistic diathesis, and to bear and require less of the stimulant treatment to which they were formerly subjected, and a more general evacuant plan. We wish to persuade ourselves that this view of the subject is true, for the credit of the art; for if it is not, the practice of medicine must be a farce, or the former practice must have been productive of extensive injury. We are certain, at least, that in most of our febrile diseases, the dread of *typhus* and malignancy is *now* a bugbear, and that the cordial treatment is generally manifestly injurious.

The judicious author of this treatise has stated, in the most philosophical manner, the series of facts which the occurrence of an epidemic brought under his observation, adopting no theory, and using no hypothetical phrases, respecting the nature and seat of the malady, but giving a plain and inductive comment upon the obvious results of the expedients resorted to, and the natural course of events when these expedients were omitted. Insomuch that, as we have already observed, his evidence amounts as near to demonstration as experiments in pathology will admit of; and we can scarcely dissent from his inference, that much mistake has been committed in supposing that puerperal fever, when epidemic, was to be treated, in any *instance*, in a different manner.

It would be difficult to do justice to the view, which Mr Hey has given of the subject, by any analysis of the volume before us, which we recommend to every practitioner to study, who may be liable to be called upon to treat this disease. For the disease is too rapid to admit of delay and hesitation, and must be attacked with as much promptitude as vigour, in order to be combated with success; and therefore the practitioner cannot have premeditated too much on the treatment which he will adopt.

The author begins by proving, in his preface, by quotations from the most distinguished authors on the subject, the extraordinary contrariety of opinion which has always prevailed respecting the disease in question, except with respect to its danger and fatality, which is generally admitted. When the epidemic at Leeds appeared, Mr Hey had not seen the excellent treatise of Dr Gordon, of Aberdeen, who adopted a different and much more successful practice than had previously been used. The sequel of the volume exhibits the early fatality of the disease under Mr Hey's management, his gradual and judicious alteration of measures, and his ultimate arrival at the same successful treatment which Dr Gordon had recommended, under which he cured a long series of cases, some of them of the most alarming kind, without losing a single patient. This detail is extremely interesting, and displays in a high degree the candour, zeal, humanity, industry, and intelligence of the author, to whom, it is obvious, many of the females of Leeds and its vicinity are indebted for their lives and safety under that formidable epidemic.

In the first introductory chapter, Mr Hey states the pathognomonic symptoms of the disease, to be "fever in child-bed, accompanied with pain which has no complete intermission, and extreme soreness, in the abdomen;" and then adverts to the different views which the most popular writers have entertained of the nature of the puerperal fever. While a few have considered the disease as purely inflammatory, others have supposed it to be a modification of typhus or jail-fever; and others again have adopted a middle course, and believed it to be inflammatory in its commencement, but to have a strong and rapid tendency to putrescence in its progress. Dr Clarke, and most other writers have generally deemed the epidemic form of the disease to be of the typhoid kind, and on that account more fatal. Dr Clarke especially distinguishes the "low fever of child-bed, connected with affection of the abdomen, which is sometimes epidemic," from the inflammatory diseases of the uterus, ovaria, and peritonæum; and Dr Thomas, who professes to give a summary of the opinions of the most celebrated writers of the present day, in his "Modern Practice of Physic," states, that the puerperal fever "has generally a strong tendency to the typhoid type," and requires an essentially different treatment from inflammation of the uterus, peritonæum, or omentum. This, indeed, is the general opinion, we believe, of the profession, resulting from the precepts of those who take the lead in directing public opinion on these subjects. These precepts had no inconsiderable influence on the first attempts of Mr Hey to cure the epidemic which appeared at Leeds. From a diligent consideration of the circumstances, there was no room for doubt that it was most similar to the "low fever" of Dr Clarke, and to that of "a typhoid type" described by others. At first every case seen by Mr Hey terminated fatally, with great rapidity. He found by experience, however, that purging gave great relief, though still fearful of carrying it to a great extent, and he did not venture upon the use of the lancet, in a disease "attended by such sudden and early sinking, and in which both these remedies had been so strongly condemned." The complete success, however, which attended the change of practice which he ultimately adopted, led him to the conclusion, that there is no essential difference in the epidemic and sporadic disease, except in the greater violence and severity of the former, and the more active employment of the same remedies by which both are cured.

In Chapter II. the author gives a sketch of the progress of the epidemic at Leeds, and a very perspicuous history of the symptoms, as they fell under his observation. The disease commenced in November 1809, and continued, with some considerable intermissions, till Christmas 1812, after which the author was prevented from seeing any more of it by a long illness in his own person. It attacked all ranks of women indiscriminately, not only in Leeds, but in the country around it, and in towns at some distance, and " many fell victims to the disorder, whose situation, circumstances, and precautions might have been expected to exempt them from it, if any care could have availed them to that end." Yet no connection could be traced between this epidemic and any perceptible variations of the atmosphere; for " it prevailed equally in cold and hot weather, in wet and dry seasons, in winter and summer." There was no other disease particularly prevalent at the same time, except erysipelatous inflammations, which also, according to Dr Gordon, accompanied the progress of the puerperal fever at Aberdeen. The occurrence of this fever was not at all connected with any particular species of labour; nay it most frequently occurred after the most easy and natural labours.

We need not enter into the detail of symptoms, which are copiously and distinctly described; but we shall quote the following passage, as throwing some light on the nature of the disease, and as comprising observations that are confirmed by all the cases hereafter recorded.

> " A degree of fulness in the hypogastric region was often evident from the first attack, and not unfrequently *the uterus* could easily be perceived, forming a distinct tumour above the pubes. *Pressure upon it gave exquisite pain.* In about six or eight hours, if the patient was not relieved, the swelling began to extend itself to the whole abdomen, which was soon distended to a great size, and the enlargement of the uterus was lost in the general tumefaction. A diminution in the size of that viscus was a very favourable symptom. The soreness and swelling of the abdomen occasioned great shortness of breathing, and obliged the patient to lie constantly on her back. There was always some mitigation of the disease when the breathing became slower, or the patient was able to change her position, and lie upon her side."

That this is a description of abdominal inflammation, beginning probably in the uterus, and extending to all the duplicatures of the peritonæum, who can doubt? The symptoms recorded in the next paragraph have, indeed, misled the best observers into a notion of *typhus*; but by the test of, we may almost say, unvarying success, Mr Hey proved that these *typhoid* symptoms are anticipated by the lancet and copious purgation.

> "If the disorder was not checked, great depression of strength and other appearances of sinking quickly supervened. The pulse was too rapid to be counted; the tongue sometimes, though not usually, became dry and brown, and the teeth were covered with sordes; the cheeks were flushed; the countenance was wild and expressive of great distress; and the whole body was covered with a clammy sweat. At this period the violent pain of the abdomen often ceased; but its distention occasioned pains in the back, sides and chest, sometimes accompanied with spasmodic paroxysms of dyspnœa. The patient became restless, and affected with vomiting, hiccough, delirium, and other symptoms which are the usual harbingers of dissolution, though not peculiar to this fever; and the melancholy scene was usually closed in a few days from the commencement of the attack." p. 24-5.

After a series of valuable practical observations on the variations in the periods of attack and termination, and of different symptoms, particularly of the rigor, pain, state of tongue, affection of the head, and condition of the blood when drawn, the author proceeds, in the third and longest chapter, to the detail of cases, which are interspersed with many practical remarks, and followed by a general view of the method of cure. The progress of his opinion in the treatment of the malady is traced in a very interesting manner. After the relation of two fatal cases, in which, as well as in all that had been mentioned to the author and his father in the practice of others, the want of success had led them to meditate before-hand upon the treatment to be adopted, in the next case that should occur, the author states the views which they took of the subject. Great temporary relief had been produced by purgatives in the two preceding cases; the stools were slimy and dysenteric; and there was obviously abdominal inflammation, "which seemed, however, of a kind not likely to bear blood-letting." They determined, therefore, to adhere more closely to Dr Denman's plan, of purging freely in the day, and procuring repose at night by an opiate; and also to evacuate the bowels early in every case by a gentle laxative, as the means of prevention. The "undesired opportunity" of putting this suggestion to the test soon occurred.

A lady was attacked with shivering, pain, and heat, about forty-eight hours after an easy delivery. The progress of the disease is fully detailed, reports being given daily, and great relief was obviously obtained from free purgation with manna and sulphate of magnesia, with intermediate anodynes, and fomentations of the abdomen. The operation of the purgatives was followed again and again by diminution of the fever, and even of the abdominal tenderness on pressure, and two quiet nights were produced; but on the third day, the symptoms again recurred, and continued to increase, with vomiting; and on the fourth, the pain and soreness were nearly gone, and other symptoms of commencing gangrene indicated the approach of the fatal termination, which took place on the following day. The fourth case we shall quote, on account of its brevity.

> "The circumstance most worthy of remark in the fourth case, was the early and sudden transition from symptoms of active inflammation to those of debility and sinking. The patient was a robust woman, who was lying in of her first child. She was delivered on Sunday afternoon at four o'clock, and on following day, she was directed to take, at intervals, a solution of sulphate of magnesia, as a means of prevention.
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> "At two o'clock on Tuesday morning she was attacked with the disease, and I was called to visit her about five. The pain was more severe than in any of the former cases; and was accompanied with a full strong pulse. The sulphate of magnesia had not *operated.* I prescribed a purging draught with sulphate of soda *and manna* āā ℥ss. in two ounces of infusion of senna. I also directed a cathartic injection to assist its operation. A consultation with a physician took place in the forenoon, when eight or ten leeches, and afterwards a blister, were ordered to be applied to the abdomen.
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> "*At three p. m.* the pulse had evidently begun to lose its strength, and other symptoms of debility had become manifest. A distention of the abdomen, which had previously taken place, advanced with rapidity; an obstinate vomiting supervened; and the disease finished its career in thirty-five hours from its first seizure.
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> "It wll not be thought surprising, by those at least who consider the puerperal fever as a modification of typhus, or as having a strong and rapid tendency to it, that the result of this case tended to confirm our fears of having recourse to the lancet." p. 51.

The 5th case was still more rapidly fatal; for the disease commenced twenty-six hours after delivery, and notwithstanding the patient was attended four times that day, and consultations held, she died forty-three hours after that event, and about seventeen from the commencement of the disease. The sixth case ended fatally in about forty-eight hours; and in this, some camphor mixture and bark were prescribed in consultation. In the seventh there appeared to be great predisposition to the disease during the latter part of pregnancy. Purging materially alleviated the symptoms at first, but an erysipelatous inflammation attacked the nates and pudendum on the third day, and on the sixth the patient died. In the eighth case the disease came on so late as six days after delivery, and copious purging was the remedy resorted to, together with anodyne clysters to soothe the irritation which it might occasion; and this patient with some difficulty recovered. It was remarkable, that " though the bowels were freely evacuated on the day after delivery, and never afterwards became constipated; and though, after the attack of the disease, purgatives and clysters were given every day, yet a remarkable quantity of hardened feces was repeatedly discharged."—" On a view of the last case," the author adds, " I am persuaded that, had we not been too fearful of exhausting the strength of the patient, but had purged her more briskly at first, the disease would much sooner have been brought to a close." p. 77. Soon afterwards two other cases were successfully treated chiefly by purging; but after this time four were attacked by the disease fatally.

Of the fourteen cases then here mentioned, which occurred in the author's practice between December 1809 and June 1810, *eleven* terminated in death, and *three* in recovery. The last case which he saw had exhibited considerable appearances of inflammation; purging had been the only remedy from which he observed any relief; and having seen an abstract of Dr Gordon's practice in Thomas's Modern Practice of Physic, in which bleeding and purging were the remedies depended upon, Mr Hey now felt strongly inclined to try the use of the lancet, notwithstanding the general impression respecting the low and typhoid type of the disease. A stout woman was attacked with symptoms of puerperal fever (Case ix.) on the third day after labour, which had been accompanied by considerable hæmorrhage. After using a purging medicine and clyster, he took away seven ounces of blood at 5 *p. m.* and applied a blister; at 8 *p. m.* the blood was found highly firm and buffy, and seven ounces more were taken, which proved more firm and buffy than the former; at 10, great relief to all the symptoms had been produced. This relief continued all next day, copious alvine evacuations being also produced, and till the evening of the following day, when a violent affection of the head came on, with strong quick pulse, which was relieved by leeches to the temples, and by the abstraction of three ounces of blood from the temporal artery. On the following day she continued better on the side of the head which had been bled, and three ounces were taken from the other, and an ounce and half more in the evening, in consequence of a relapse. On the next day some symptoms of paralysis shewed themselves, and in consultation some wine and an etherial medicine were ordered. There was no farther abdominal affection; but the patient became delirious, and died on the 13th day in a state of great distress and restlessness. This case was in fact a remarkable instance of metastasis; but the entire removal of the abdominal affection, and the appearance of the blood, confirmed the author in his notion of the propriety of blood-letting. In the 10th case, which occurred soon after, he took away 20 ounces of blood, seven or eight hours after the disease commenced, and found the pain diminished while the blood was flowing, and the next morning nearly gone; purging was continued two days, and the patient recovered without further complaint.

From this time the author adopted Dr Gordon's method, (whose treatise he had now obtained,) and which coincided with, and confirmed his own recent views; and after this period he attended a great number of patients, attacked with equal or more violence than those whom he had already treated, of whom, however, but three died, namely, in the following order, the 11th, 13th, and 15th cases; but after this period, by a still more vigorous pursuit of the same plan, he had the satisfaction of seeing *all his patients recover*. In the three fatal cases, indeed, the circumstances were such as to afford no disparagement to the principle of the treatment employed: in the 13th case, venesection was not permitted; and in the 15th, the disease was so much subdued, that the patient went on to the end of six weeks, and perished rather from the sequelæ, than from the actual disease. We quote the following case (the 21st) as a specimen of the author's active and successful management of this previously fatal disease.

"August 3d, 1812, at one o'clock in the morning, the wife of T. W. of Hunslet, a woman of rather delicate appearance, was delivered by a midwife of her 12th child, after an easy labour of about an hour. Her discharge both at the time of labour and afterwards was said to be severe, but not excessive. On the following morning she had a shivering fit, which was not, however, succeeded by pain; and she remained well throughout the day. The after-pains were slight.

5th.—At four o'clock in the morning, she was suddenly seized, without any previous chilliness, with a violent pain in the body, resembling labour-pain, but of much longer duration. It increased progressively during the day; and in the intervals, which were not longer than a quarter of an hour, the abdomen was sore.

"I first saw her between four and five in the afternoon, and found her crying out in pain like a woman in labour, &c."——"The patient had taken some opening medicine, which had produced one loose evacuation in the morning. The symptoms, in this case, were not the most alarming, considering that thirteen hours had elapsed since the commencement of the disease; but the pain was violent, and the loss of time was more than a counterbalance to the apparent mildness of the other symptoms. I was therefore satisfied that large bleeding in the first instance was necessary; especially as night was approaching, and the patient lived at some distance from me. I first took away twenty-five ounces of blood, without producing any degree of faintness; when I closed the orifice for a few moments, till another basin was procured; and then drew nine ounces more. She was now disposed to faint, and the pain was much diminished. I put my finger on the orifice, and waited a while. The faintness soon went off, and the pain returned; I therefore took away six ounces more, making in the whole forty ounces. The patient becoming again very faint, I tied up the arm: she soon recovered, and remained easy. Pulse 88." (it was "112 and hard" before the bleeding.) "A clyster was injected as soon as it could be prepared, which in ten minutes produced a very copious evacuation of solid feces. At six *p. m.* I gave a bolus with half a dram of jalap and four grains of calomel; and left directions that three table spoonfuls of the cathartic solution" (water eight ounces, sulphate of magnes. one ounce) "should be taken every two hours, till the bowels should be well opened, beginning two hours after the bolus.

"*Half-past ten, p. m.* The pain had returned soon after I left her, and with as much severity as before the bleeding. She had had three small watery stools, which did not appear to be the effect of the purgatives. The heat of the skin was now considerable, and was attended with much restlessness. The pulse was at 120, and still hard. The tongue was rather white, and the abdomen was much more tender; particularly in the region of the uterus, which had become enlarged, and easily distinguishable. The increase of all the symptoms since my former visit seemed not only to justify the quantity of blood then taken, but to require a further evacuation. I tied up the arm, and took eight ounces from the same orifice, when, the patient proving faint, I desisted. The pain was much alleviated by this second bleeding, and the pulse came down to 84. I ordered the solution to be taken every hour.

"6th. *Eight a. m.*—She had remained nearly free from pain all the night; the soreness had greatly abated, the uterus was diminished in size, and she had slept several hours. The skin was moist, and of a natural heat; the pulse at 100. She had taken above two ounces of magnes. sulphas, besides the purging bolus; and had had many small evacuations, which, however, contained but little feces. Two boluses were therefore prescribed, with fifteen grains of jalap and two of calomel in each, to be taken with an interval of two hours; and the solution was ordered to be afterwards continued.

"*Six p. m.*—Both the boluses had been taken, and the remainder of the third ounce of magnes. sulphas, which had procured a great number of natural stools. The patient continued free from pain; the soreness of the abdomen was quite gone, and the uterus was scarcely to be felt.

"7th.—She had slept the greatest part of the night, and the pulse was at 84. The bowels were kept open, and she continued convalescent."—p. 131.

We cannot help remarking, on the perusal of this and most of the cases here related, how very easy it would be for any treatment of this sort to lose its credit; and how very unlikely that it should ever gain credit, against the prejudices of education, in ordinary hands; because its failure to cure, when timidly used, or when resorted to late, or by men who did not thus assiduously repeat their visits at intervals of a few hours, in proportion to the danger of their patients, would probably be construed into actual injury. And it has been thus perhaps from the inefficient influence of a small bleeding, begun too late, or repeated after too long an interval, that the natural tendency of the disease to rapid lowness may have been supposed to be the result of, or at least to have been aggravated by the ill-managed remedy. The necessity of proportioning blood-letting, in all cases, to the actual effect which it is observed to produce upon the patient and the disease, and not by any arbitrary measure of ounces, if we would do justice to the patient, and obtain the full agency of the remedy, must be very obvious; and it is perhaps to this assiduous and judicious use of the means, that the great success of Mr Hey is to be chiefly imputed.

In the conclusion of this chapter, there are many excellent practical observations, which the length to which we have already extended our remarks will not permit us to recapitulate. Bleeding and purging, when employed in a proper and seasonable manner, were obviously adequate to the cure of the disease, and other remedies were merely auxiliary. Dr Gordon affirmed, that one bleeding of 24 ounces, if performed within six or eight hours of the attack, together with a single purgation, never failed to cure the disease; that if called within 12 hours he promised success from bleeding; but if the period were later, as from 12 to 24 hours after the attack, he could not promise success, though he deemed it the only effectual remedy. Mr Hey justly maintains that no such precise limit can be laid down in all cases; for while one of his cases terminated fatally in less than 18 hours, in some others bleeding was successful, when a delay of more than 24 hours had occurred. Nor does he admit any similar limitation of the quantity of blood to be drawn: he thinks, indeed, that there is scarcely any other limit than the removal or considerable diminution of the pain, "provided all that is requisite be drawn within twelve hours of the first evacuation."

"If the disease is clearly ascertained, no other consideration is of much importance. The state of the pulse affords little information either as to the propriety of bleeding, or the quantity of blood proper to be taken away; and if we are deterred, either by the apparent weakness of the patient, by the frequency and feebleness of the pulse, or by any other symptom, from bleeding copiously, we shall generally fail to cure the disease." p. 161.

Immediately after bleeding, the most speedy and most effectual means of purgation were resorted to, and continued, so as to produce an evacuation once in three or four hours, which was maintained for two or three days, or longer if necessary. This accords with Dr Gordon's precept, that "the purging is to be early excited, and to be continued without intermission, till there be a complete termination of the disease." Mr Hey always found *opiates* destitute of any advantage, affording but an insidious truce, and rather tending to prolong the disease. Circumstances may render purgative or emollient injections useful; fomentations, if properly applied, are soothing, and never did harm; but blisters are inconvenient, and seldom necessary.

If these means fail to cure the disease, from being employed too late or in an improper manner, the mischief produced in the cavity of the abdomen, whether by extravasation, suppuration, or gangrene, is beyond the reach of medicine; and nothing can be done, save to alleviate distressing feelings; cordials and tonics can afford no other advantage. Purging, however, is proper in every stage of the disease, unless gangrene has actually taken place; and should be excited, if possible, when bleeding has become inadmissible.

The success of this practice, then, was as follows. Before the plan of bleeding was adopted, of fourteen patients only three recovered; but after the 15th case, which occurred in the author's practice, of thirty-three patients only three died; and the last twenty-six recovered in uninterrupted succession. Yet this was an *epidemic* puerperal fever, which has been said by most of the writers to be of a low or typhoid character. This is at least the third example of an epidemic fever of this sort, which was combated effectually by copious and active evacuations only; for those of Aberdeen and Sunderland, described by Drs Gordon and Armstrong, were of the same species. When we consider what a general bugbear typhus, (*i. e.* debility, putrescency, &c.) was, at the time when most of the books on this subject were written; how distinctly in some cases the general opinion has veered round in that respect of late; how very much like low fever are many of the abdominal inflammations after the acute stage is past, where they have been neglected or maltreated; and how injurious we find the use of cordials, bark, and other excitants in the common fevers of this country at present; we are disposed to agree with the author before us, that the puerperal fever is always essentially the same disease, "inflammatory at its beginning, and putrid only in its progress;" and that the distinction between the *sporadic* and *epidemic* disease, is so far only important in practice, "that the epidemic disease requires more prompt attention, and more vigorous treatment;" for that "the means of cure are precisely the same in both; but in the latter their measure is greater and less limited, and the period within which they must be employed is far more circumscribed." p. 182.

Whatever may be the general inference upon this subject, this little treatise will be deemed a perspicuous and judicious history of a severe and dangerous epidemic, and characterized by sound pathological knowledge, clear arrangement, and great correctness of composition, and worthy to be classed with the best monographs of modern medical literature.

V.

A Treatise on Fever, with Observations on the Practice adopted for its Cure in the Fever Hospital and House of Recovery in Dublin. Illustrated by Cases. By WILLIAM STOKER, M. D. one of the Physicians of that Institution, and Licentiate of the King and Queen's College of Physicians in Ireland. 8vo. pp. 198. London, 1815. Longman and Co.

THIS is not a formal systematic treatise on fever, entering into the common discussions about the causes and nature of febrile affections, and their various modifications. It consists, in fact, principally of a sort of journal of the cases of fever, which occurred under the author's observation, during a short period of his attendance at that extensive institution. This constitutes at least the greater portion of the volume; and to it are prefixed some remarks on the different remedies employed in the practice there, and a preface, containing an account of the structure and general management of the hospital, with some observations on the extraordinary prevalence of fever in Dublin, in comparison with the metropolitan cities of the sister island, as well as on its more severe and malignant type. The author ascribes much of this greater prevalence of typhoid contagion to the wretched and crowded state of the habitations of the poor, and the filthy approaches to them; but more, apparently, to "the brutalizing use of whisky," which he describes as so habitual to the lower classes, "that they pass their lives between alternate intemperance and famine." (p. xxi.) This may undoubtedly predispose to febrile diseases; but the more obvious exciting causes of these maladies are perhaps to be found in the former circumstances. We know that in London, the courts and alleys, occupied by crowds of Irish labourers and their families, which are proverbially filthy, both in the interior and in the approaches, are almost exclusively the seats of the small number of fevers that now occur in that metropolis; and although their intemperance in the use of gin is also notorious, yet it is probably much less habitual than in Dublin, according to Dr Stoker's description. The scavengers might possibly do much to amend that condition; but improvement in education, and a gradually increasing sense of the value of cleanliness, air, and other domestic comforts, which increasing intelligence, and its companion, sobriety, can alone perhaps be expected to introduce, are probably the sources to which we are to look for a radical amendment.

The author commences the work itself with some general observations on the advantages of hospital practice for the attainment of medical truth. We doubt, however, whether, on the whole, the hurry and rapidity which a great number of patients occasions, be so consistent with careful observation as the more quiet attendance upon the bedside of private individuals; and we doubt, whether the higher responsibility, in the latter case, the anxiety and importunities of surrounding friends, and the greater benefit or injury accruing to the reputation of the practitioner, from success or failure, do not in general call forth more assiduity in investigating symptoms, and more nicety in marking the diagnosis and prognosis, of which a daily annunciation is required, and therefore, whether a more accurate knowledge of diseases is not thus generally attained in private practice. A register of cases and their progress, indeed, is seldom kept, except in hospital practice; and this is certainly a valuable source of information, by the means of subsequent reference and comparison which it affords. One of the results obtainable from such a register, Dr Stoker has noticed, viz. an estimate of the tendency of fevers to decline on particular days. A little table is given of the days of decline in 1773 cases of fever, which exhibited amendment on various days from the second to the thirty-first inclusive. The greatest number, 262, is said to have taken place on the 7th day, and the next, 212, on the 9th day; on the 8th an intermediate number, 173; but on the 5th, 6th, 10th, 11th, and 12th, a nearly uniform number of about 120; from the 13th to the 31st days, the number pretty uniformly decreases from 82 to 2. There appears, then, to be a particular tendency to decline on the 7th and 9th days, and not a little on the 8th; but this table does not in any degree countenance the old doctrine of a septenary period, (for the 14th day exhibits less than the 12th or 13th, and the 21st less than the 19th or 20th,) nor of a tertian period up to the 11th day, and a quartan period afterwards, as Dr Cullen supposed, (for the 11th day exhibits a smaller number than either the 10th or the 12th, and the 17th a smaller number than the 15th or 16th.) We are not much inclined, indeed, to consider these observations, respecting the particular days of decline or termination of fevers, as entitled to much credit, on account of the difficulty which occurs, at both ends of the period, of ascertaining the actual commencement or decline of the symptoms with accuracy. The decline, especially, is rarely marked so distinctly as to be assigned to a particular period, or to assume anything like a complete crisis; and it will be much varied by external circumstances, such as, by the removal of the patient from a close and filthy habitation, to the clean and airy ward of an hospital. The decline of the fever upon such a removal is often immediate; and if this removal should take place, as perhaps it most commonly does, when the patient has been about a week ill, then we should expect the number of recoveries commencing on the 7th, 8th, and 9th days, to be the greatest in these hospital patients. Dr Stoker has remarked this influence of the change of situation; but whether his distinction of the particular days is the clear result of observation, or of a leaning to the doctrine of critical days, we know not. "In cases of unmixed fever," he says, "convalescence frequently takes place on the night after admission to the hospital, if it be on the 5th, 7th, or 9th from the attack, or on the next succeeding to it, if on any of the intervening days." (p. 9.)

In treating of the methods of cure in idiopathic fever, the author observes, that cleanliness, ventilation, cool regimen, and plentiful dilution, appear to be the only means that are universally applicable to the cure of this disease. The eight other remedies, to which it is usual to resort, are indicated under various modifications, and the author arranges them, according to his opinion of their importance, in the following order: viz. purgatives, topical bleeding, antimonials, yeast or barm, wine, emetics, cold or tepid ablution, and blisters. His observations on these agents, in their respective order, constitute the body of the work, and extend to p. 55. He prefaces these observations, however, with an explanation of the absence of cinchona from the list of remedies, having been found unnecessary, and of the low estimate of cold ablution, which was found not to be beneficial in a great many patients of the hospital, "on account of their being generally affected with, or predisposed to, some pulmonary disease." He objects also to the use of the lancet, as productive of great debility, even in the repeated small bleedings employed by one of the physicians to the establishment, notwithstanding the general prevalence of pulmonic symptoms.

The author's observations on *purgatives* are very brief and common-place; the point of them is comprised in a quotation from Dr Hamilton, which recommends these medicines only as evacuants of feculent matter, and not as productive of copious watery excretions. One single practical observation occurs, which seems rather scanty, as the result of "an experience of nearly ten years in an hospital, wherein fifteen thousand one hundred and sixty-four patients were received during that period." (p. 16.) This solitary remark is, that "very obstinate constipation of the bowels occurs when the head is much engaged in fever; and purgatives, which have no effect on patients in a state of delirium or stupor, will, after the head is shaved, or blood taken from it, operate freely." (p. 27.) There are many cases of fever, we believe, in which the head appears to be very moderately "*engaged*," and the intestinal canal, stomach, or chylopoetic organs, appear to be much deranged in their functions, and which are apt to be tedious of cure, and in which great choice and caution in the use of purgatives has appeared to us to be necessary; and where diarrhœa is troublesome, which now and then happens, in fevers, which, though perhaps not to be called typhoid, are brought into these hospitals, in which the question of using or abstaining from purgatives, and the choice or combination, is a matter of difficulty. We should have been glad of some information on the varieties and exceptions, as to the use of purgatives in particular states of fever, in such a field.

The remarks on the use of *local blood-letting* are equally brief and general, and consist in recommending the use of leeches and the section of the temporal artery, in preference to cupping. With respect to *James's powders*, the author appears to confide in their efficacy very implicitly, ascribing to them some occult power of relief, by which they "will often appear to promote the efforts of nature towards health," and which their evacuant operation will not explain. He appears to consider them as directly sedative, especially in controlling the circulation of the brain.

> "In the commencement of mixed cases of fever, in which evacuants have been employed as far as the warning symptoms of debility admit of, without removal of the inflammatory action of the vessels, and in the advanced stages of slow fevers attended by parched skin and quick pulse, there is no remedy perhaps of superior efficacy. The discharge which they excite will not alone account for their *modus operandi*; for equal discharges caused by other means are not succeeded by proportionate relief, and sometimes the pulse is reduced both in frequency and hardness, feverish anxiety diminished, and sleep induced during their administration, though no such adequate evacuation be observable.
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> "Either alone, or in conjunction with preparations of mercury, they are now chiefly confided in by the reporter, in tendencies to effusion on the brain in fever, when evacuants fail in removing that train of symptoms; and perhaps it is only from their use that any reasonable hope may be entertained, when effusion has actually taken place." (p. 43.)

The author speaks of *barm* or *yeast* as an useful remedy, well suited to every stage of typhus fever, sitting easy on an *irritable* stomach, being moderately laxative, correcting symptoms of putrescence, petechiæ, and black tongue, and being therefore his usual substitute for the common antiputrescents, wine and bark. We have no doubt, though we have never prescribed the remedy, that it has more efficacy in such cases than wine and bark, which have always appeared to us to make a black tongue blacker, and to take nothing from the number or hue of petechiæ. Probably, however, Sydenham was right in supposing that particular *constitutions* exist in different epidemics, so that fevers may at different periods require different modes of treatment: we cannot otherwise account for the varying practice of eminent physicians, (in respect to the former copious use of bark and wine, and the late proscription of these substances altogether,) "qui ad eandem tamen sanitatem homines perduxerint."

The author animadverts upon the varying opinions with respect to *wine*, which he ascribes to an indiscriminate and injudicious use of it. He justly condemns the early administration of it, as well as its exhibition in cases of typhus complicated with other diseases. Wine is rarely directed in the Dublin hospital in those cases, (which constitute the majority) that terminate before the tenth day; and the author candidly admits that the use of wine, even in those of greater duration, has been in the inverse ratio of the experience; for he says, "the quantity given in the more protracted forms has been gradually lessening since the opening of the hospital;" (p. 47.)—an admission which we believe all candid and observing physicians will be disposed to make in regard to their own experience. On examining the diet-books, " the author finds that he has not directed it (wine) in more than half as many cases in proportion to those committed to his care, in the latter years as in the former; and that even in those, for whom he did order it, the quantity seldom exceeds one half of what he formerly deemed necessary." He contents himself, however, with quoting three pages of directions from Dr Wilson Philip's Treatise on Fever, respecting the administration of wine, which, he says, are consonant with his own experience. They are doubtless highly judicious; and the cautions of that intelligent writer, with respect to watching carefully its effects, and withdrawing it where any over-excitement obviously results from it, even in the most protracted stages of typhus, cannot be too minutely observed. We have seen a fatal excitement follow a depression, that had threatened to be fatal, and had been removed by the stimulation, from the injudicious perseverance in the stimulus. The balance, indeed, in such a case, is very difficult to preserve: for the excitement is liable to become as excessive as the previous depression, if the exciting cause be not diminished or withdrawn. We are disposed to admit with great limitation the concluding remark of the author, " that if, in the protracted stages of fever, evacuations are indicated for the relief of local obstruction or turgescence, the employment of them need not prevent the administration of wine, proportionate to the general debility, and it will often be found that these remedies, though productive of such opposite effects in the commencement, will, towards the termination of protracted fever, materially assist each other in promoting the recovery of the patient." (p. 51.) If there is a very active congestion in the head, for example, or in any organ of importance, it is liable to be aggravated by any general stimulus, and the principle seems to be, in such a case, to support, but to avoid stimulation.

On the subject of *emetics*, the author seems to be impressed with a groundless apprehension of mischief produced by tartar emetic, and even by ipecacuan, which induces him to recommend copious draughts of warm water, or of a tepid infusion of chamomile, in the commencement of fevers, which, he says, seldom fail of being beneficial: but if these fail to excite vomiting, he trusts to the more natural means of evacuation by purgation. *Blisters* are employed only near the parts affected, and therefore principally about the head and neck, in cases where coma threatens or supervenes, and never in distant parts, with a view to the notion of revulsion. They are considered by the author merely as *stimulants*, and therefore not employed in the *early* stages; and he deems that they relieve coma, by exciting the vital energy, to the diminution of which he ascribes "the suppression or diminution of any natural secretion, critical discharge, or eruption on the skin, with which the coma seems often connected." But, according to this principle, and to another hypothetical notion of the author, that "probably they are also serviceable by the practical absorption of the cantharides, which are thus carried directly to the vessels concerned in impeded secretions," it would be of no importance to what part the blisters are applied. Stimulation would be excited, and absorption would take place, if the blisters were applied, in the old humoral style, to the arms and legs, as well as when they are applied near the affected parts. The theory is at variance with the practice, which, however, is supported upon the stronger basis of observation. For, whether we admit somewhat of the doctrine of revulsion, which we are disposed to consider as more accordant with the facts, or not, we believe that as little benefit will be derived, in a commencing febrile stupor, with ferrety eyes, from blistering the legs, as would be obtained in an inflammatory affection of the bronchial membranes by blistering these distant parts; and in the latter case, a blister on the chest could scarcely be said to relieve the local internal inflammation by exciting the vital energy.

The doctrine of revulsion, however, in its old and extended sense, appears to be acted on by the author in respect of another remedy, which is not noticed in his general observations, but is often prescribed in the cases detailed; we mean the *pediluvium*, or rather *warm fomentation* of the feet. This is employed when the head is much affected, and in conjunction with the application of leeches, with bleeding from the temporal artery, and with cold applications to the shaven scalp. Now we have never been able to persuade ourselves that the use of warm fomentation to the lower extremities could operate in any other way than as a partial warm bath, *i. e.* as a considerable stimulant of the general circulation, and of the circulation of the head as a part of the whole. The notion of revulsion between parts so distant, is justly scouted by the author. Indeed we believe the theory, upon which this practice has been recommended, to be bad, and the practice to be at least of very doubtful propriety, where any local congestion is manifest.

On the whole, however, the practice recommended in this work, and illustrated by a detail of the progress of about a hundred cases, from the register of the hospital, must be deemed highly judicious, and worthy to be followed. It is the practice into which we are sure that every attentive and unprejudiced physician would be compelled, by a similar range of experience, to bring himself by degrees, whatever might be the doctrines of debility and putridity, with which he may have been tainted in the schools, and by the writings of the past age, and in whatever terror he might have consequently held the free use of purgation, of cold, and topical bleeding, or in whatever estimation the powers of cinchona, and wine, and other diffusible stimulants. There is very little labour required, indeed, for the formation of such a book, three-fourths of which consist of the journal and pharmacopoeia of the hospital; and the preceding remarks are both brief and general, betokening no particular care or skill in composition, and little or no attempt at method or arrangement. But perhaps, after all, the value of a publication is not to be measured by the labour with which it was composed; and it may be doubted whether any better mode of instruction, on the subject of a practical art, can be devised, than a faithful and simple detail of examples of a particular disease, judiciously treated by an assiduous and intelligent physician. Such we deem to be, on the whole, the character of this ample detail of practice, given to the public by Dr Stoker; and we recommend it to those who want the sanction of experience to confirm their half-established conviction of the value and success of this rational treatment of the fevers of these climates.

VI.

Experiments made with a view to ascertain the Principle on which the Action of the Heart depends, and the relation which subsists between that Organ and the Nervous System. By A. P. Wilson Philip, Physician in Worcester. From the Philosophical Transactions of 1815.

Dr W. Philip began his experiments with a view to ascertain the *modus operandi* of certain poisons; but, finding it impossible to arrive at an accurate knowledge of this subject, without a previous acquaintance with the principle on which the action of the heart depends, and the relation which subsists between the sanguiferous and nervous systems, and that the best writers on these subjects contradict each other, he was led to the experiments and observations which we are about to consider. It seems surprising that, on subjects of such importance, and on which so much has been written, there should still be so much room for inquiry.

By the earlier writers it was believed that the heart, as well as every other part of the system, derives its power from the brain, and Haller was the first who, by decisive experiments, proved this opinion to be erroneous. He shewed that the heart is capable of performing its functions after the brain is destroyed; and, indeed, in the acephalous fœtus, we find it performing them where the brain has never existed. From his experiments, he was led, perhaps too hastily, to conclude, that the heart is wholly independent of the nervous system. This opinion was adopted by many; and Bichât, in particular (*Recherches Phys. sur la Vie et la Mort*,) has made various experiments to prove that the heart cannot be influenced through this system.

The opinion of Haller, however, has never been universally received. It has always been objected to it, that the heart, like other parts of the body, is supplied with nerves, and that its action is evidently influenced by affections of the mind. It is needless to say any thing of various opinions which have prevailed on this subject, and of the attempts which have been made to reconcile the doctrine of Haller with the phenomena which oppose it. They can be regarded in no other light than ingenious hypotheses. They are chiefly founded on certain supposed properties of the nerves of the heart, operations of the ganglia, &c.

While we seemed to be relapsing into all the uncertainty which preceded the experiments of Haller, a writer of celebrity brought forward a very extensive set of experiments, which seemed to throw a new light on the subject, and wholly to set aside the opinion of this great physiologist. M. le Gallois, in his Treatise *sur la Principe de la Vie, &c.* admitting that the heart is independent of the brain,—has maintained that it derives its power from the spinal marrow, particularly the cervical part of it; and the experiments supporting this opinion he repeated before a committee of the National Institute at Paris, who published a declaration expressive of their conviction of their accuracy.

It appears, however, from what is said in the latter part of Dr Philip's paper, that there are great apparent inconsistencies in the results of M. le Gallois' experiments; for, although there can be no doubt that, in some of them, the power of the heart was destroyed by the destruction of the cervical part of the spinal marrow; it appears from others, that the same, or a greater part of the spinal marrow, may be destroyed in a different way, without materially affecting its power. M. le Gallois' explanation of this apparent inconsistency, Dr Philip has shewn to be not only irreconcilable to fact, but inconsistent even with the opinions maintained by M. le Gallois himself.

M. le Gallois found that, when the cervical portion of the spinal marrow is *at once* destroyed, the power of the heart is immediately lost; but that this, as well as any other part of the spinal marrow, may be destroyed by small portions at a time, without materially affecting the action of the heart. That these are the facts Dr Philip admits, and, consequently, that Haller's doctrine, in the extent to which he and his followers carried it, is set aside. M. le Gallois thus attempts to reconcile these facts. He found that, by throwing ligatures round the vessels at some distance from the heart, thus narrowing the circulation, the action of the heart can support it under circumstances in which it would otherwise be lost; from which he infers, that, when the heart is found still to support the circulation, after the cervical part of the spinal marrow has been destroyed by small portions, this arises from the circulation being rendered very languid in the parts of the body which derive their nerves from these small portions of the spinal marrow; and, consequently, from the circulation being narrowed, as it is when ligatures are thrown round the vessels in the way just pointed out. The following are Dr Philip's observations on this explanation:

> "Now, although it were ascertained that the circulation is impeded in any part by destroying the portion of the spinal marrow, from which it is supplied with nerves, which I think may easily be shewn not to be the case," (it is proved, indeed, not to be the case by several of Dr Philip's experiments, which we shall have occasion to mention,) "this explanation would still be in opposition to M. le Gallois' fundamental position, 'Que la quantité, que le contingent de forces, que chaque portion de moëlle fournit à cet organe, egale, pour le moins, celles dont il auroit strictement besoin pour entretenir la circulation dans les seules parties correspondantes à cette portion.' When the ligatures were thrown around the vessels, the heart was deprived of none of its supposed nervous influence; when, on the contrary, portions of the spinal marrow were successively destroyed, as far as this is supposed to confine the circulation, it must also, according to M. le Gallois, occasion a loss of power in the heart. He remarks, that, till the above explanation occurred to him, he had resolved to abandon this part of the inquiry. 'Après bien des efforts inutiles pour porter la lumière dans cette tenebreuse question, je pris le parti de l'abandonner, non sans regret d'y avoir sacrifie un grand nombre d'animaux, et perdu beaucoup

de temps.' Just before he observes, 'En un mot, j'eus presque autant de resultats differens que d'experiences.'"

The confusion arising from these seeming contradictions is further increased, by M. le Gallois having found, that, if the spinal marrow is divided about the middle, the power of the heart is destroyed by the destruction of either half,—a fact which, as Dr Philip observes, is in direct opposition to M. le Gallois' explanation of the foregoing difficulty.

Thus we see the subject still involved in obscurity; and the facts ascertained by M. le Gallois, highly important as they appear to be, from their seeming irreconcilability, tend to produce a greater apparent confusion in this subject than existed previous to the publication of his experiments. We are to consider how far the experiments of Dr Philip tend to remove that confusion, and to explain the various and apparently contradictory facts of which we are now in possession. We shall take a cursory view of the whole of his experiments, in the order in which they are related.

In his first experiment, the cervical part of the spinal marrow was wholly removed, while the heart lay in view, without at all apparently affecting its action. In the second, the same part of the spinal marrow was destroyed by a hot wire, with the same result. As there was no direct proof, in these experiments, of the heart supporting the circulation after the destruction of the spinal marrow, in the 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, and 11th experiments, the cervical part, and sometimes the whole of the spinal marrow, were either destroyed or removed, and sometimes the brain also, both in warm and cold blooded animals, and the circulation found still to be vigorous. On opening the carotid and femoral arteries, either black or red blood flowed from these vessels, according as artificial respiration was performed or not; and, in the same animal, after the spinal marrow was wholly destroyed, the blood was made to flow from the vessels alternately black or red, according as artificial respiration was discontinued or renewed, proving how perfectly the heart and blood-vessels continued to perform their functions.

The next subject of inquiry in Dr Philip's paper is, how far the heart is subject to the action of stimuli, applied to the brain and spinal marrow, while these organs and the connection of nerves remain entire? After relating the eleventh experiment, he remarks:

"I have already had occasion to observe, that it is generally admitted, that the action of the heart cannot be influenced by stimuli applied to the nervous system, and it seems almost a contradiction to suppose that it should, when we see that it cannot be influenced by the total destruction of this system. There were many reasons, however, which induced me to try the effect on the heart of stimuli so applied to the brain and spinal marrow, as not to excite any of the muscles of voluntary motion, whose action, either by throwing more blood towards the heart, or in some other way influencing its action, prevents our judging of the effect of the stimulus."

In the 12th, 13th, 14th, 15th, and 16th experiments, Dr Philip finds, that the heart, both in the warm and cold blooded animals, is readily influenced, both by stimuli and sedatives, applied either to the brain or spinal marrow. The action of the muscles of voluntary motion was prevented in the warm-blooded animal, by a blow on the occiput, which deprived it of sensibility; and, in the cold-blooded animal, by immersing the hind legs in laudanum, previous to the commencement of the experiment.

In the 17th experiment, he found that the motion of the intestines, like that of the heart, is not at all influenced by the total removal of the brain and spinal marrow. He attempted to ascertain how far it is influenced by stimuli and sedatives applied to these organs. From its extreme irregularity, he could draw no certain conclusion from his experiments on this head; but he remarks, that the influence of affections of the mind on the bowels, leaves no room to doubt that, like the heart, they are capable of being stimulated through the nervous system.

In this part of the paper, Dr Philip observes,

"What are the simple results of the foregoing experiments? The first set prove that the power of the heart is independent of the brain and spinal marrow, for we find that it continues to perform its function after they are destroyed or removed, and that their removal is not attended with any immediate effect on its motions. The second set prove that the action of the heart may be influenced by agents applied to any considerable portion either of the brain or spinal marrow. It is as readily influenced by agents applied to the anterior part of the brain, as by those applied to the cervical part of the spinal marrow. This is what we should expect when we trace the various origins of its nerves. If it be said that the results of these experiments imply a contradiction, that we cannot suppose the power of the heart to be wholly independent of the brain and spinal marrow, and yet influenced by stimuli applied to them, the reply is, that such are the facts, of the truth of which any one may easily satisfy himself. Daily occurrences correspond with these facts. We rarely see the action of the heart destroyed by injuries of the brain and spinal marrow, unless they are such as interrupt respiration; yet its action is constantly influenced by affections of the mind."

Dr Philip then points out other similar difficulties which present themselves in the phenomena of the nervous system. M. le Gallois has proved that a principal function of the spinal marrow is to excite the muscles of voluntary motion, and that it can perform this office perfectly, independently of the brain; yet injuries of the brain impair the functions of the spinal marrow. We may remove the brain, and the motions of the limbs are still perfectly performed, yet an injury of the brain occasions hemiplegia.

Dr Philip's 18th and 19th experiments point out still another instance of this apparent contradiction, and seem to suggest the principle on which the whole depends. In the 18th experiment, strong and repeated contractions were excited in the hind legs of a frog, by applying stimuli to the spinal marrow as long as they could be produced. On examining the state of these muscles, they were found wholly deprived of their excitability. Yet it is well known, he observes, that although all the nerves supplying the limbs be divided, and cut out close to the place where they enter the muscles, the latter still retain their excitability, which appears to be not at all less than when their nerves are entire. Here it may be said, that although the nerves are divided, the nervous influence may still be supposed to remain in the muscles, and thus preserve their excitability. In the 19th experiment, therefore, the nerves of one of the hind legs of a frog were divided, so that it became quite paralytic. Salt was then sprinkled on its muscles as long as any contraction could be excited in them. Salt was applied in the same way to the muscles of the other leg, whose nerves were entire, and produced exhaustion in them in a shorter time than in the muscles whose nerves had been divided. Thus it appears, that although the power of the muscles of voluntary motion can be exhausted through the medium of the nervous system, the influence of this system has no effect in preserving their excitability; but on the contrary, exhausts it as other stimuli do. It is a property of the muscle itself, although capable of being influenced, and even destroyed through the nervous system.

"On the same principle," Dr Philip observes, "we explain the seeming contradiction respecting the action of the heart. We have seen that its power exists as independently of the brain and spinal marrow, as the action of the first muscles to which the salt was applied, whose nerves had been divided; but, while the brain and spinal marrow retain their functions, and the connection of nerves is entire, the heart, as well as the muscles of voluntary motion, may be influenced by agents acting through the nervous system."

It may here be objected, that, in apoplexy, the power of the heart remains, while that of the muscles of voluntary motion is lost. But Dr Philip has shown that this does not arise from any diminution of the excitability of the muscles of voluntary motion in apoplexy, for he found it entire in this disease both in warm and cold-blooded animals, but from the stimulus of the muscles of voluntary motion, the nervous influence being withdrawn, while the usual stimulus of the heart remains.

By the whole of the foregoing experiments, Dr Philip considers that part of Haller's doctrine, which teaches that the heart and other muscles possess a power independent of the nervous system, confirmed; but that part, which teaches that this system possesses no direct influence over the muscles of involuntary motion, refuted. While he regards the power of all muscles as equally independent of the nervous system, he considers all as equally capable of being stimulated through this system. The only difference being, that the one set of muscles are always stimulated through the nervous system, while the others, although occasionally stimulated through this system, usually obey a stimulus as independent of the nervous system, as the salt applied to the muscles of voluntary motion in the above experiment.

Against this view of the subject it is evident, that the objections brought against the doctrine of Haller have no weight. We see, from the foregoing experiments, why the heart should be supplied with nerves, and influenced by affections of the mind.

Dr Philip endeavoured to trace the subject farther.

> "It has been shewn," he remarks, "by direct experiment by M. le Gallois, that the spinal marrow is capable of performing its functions independently of the brain, yet, as has just been observed, the spinal marrow may be influenced through the brain. Thus the excitability of the spinal marrow bears the same relation to the brain, which that of the muscles bears to the spinal marrow and its nerves; and I would add, all nerves distributed to muscles, some of which arise from the brain, but seem to bear precisely the same relation to the sensorium with those which arise from the spinal marrow. Even M. le Gallois, although his experiments lead to an opposite conclusion, observes, that the brain seems to act on the spinal marrow as the latter does on the parts it animates. We know the peculiar office of the brain, by observing what functions are lost by its removal, the sensorial functions. The nervous thus obeys the sensorial system, in the same way in which the muscular obeys the nervous system, but as the muscular system has an existence independent of the nervous, so has the nervous independent of the sensorial system."

He regards these observations as illustrated by what we observe in the different classes of animals. In the lowest class we have evidence only of a muscular system. In the next class we find the muscular and nervous systems without sensorium. In the most perfect animals, we find the three systems combined, not immediately depending on each other, but all so connected, that none can exist long without the others. The nature of this connection, he observes, is obvious, when we consider that all are supported by the circulation, which depends for its immediate support on the muscular system, and cannot long exist without respiration, which depends not on the sensorium, but, as M. le Gallois has satisfactorily proved, on the nervous system, which is under the immediate influence of the sensorium.

Dr Philip's experiments conclude with four made, two on warm, and two on cold-blooded animals, with a view to shew that the heart, notwithstanding its power, is not independent of the nervous system; is not merely capable of being stimulated through this system, as appears from the experiments to which we have already referred, but of having its power wholly destroyed by powerful agents acting on this system. We shall quote one of the most striking of these experiments. *It shews* the difference of the effect produced on the heart by simply removing the brain, and by applying to it the most powerful agent.

"The anterior part of the brain of a rabbit was crushed by a blow. The side was rendered hard by spasm for about half a minute. Neither during this nor after it could I perceive any motion of the heart by applying the hand to the side. The head was then cut off about three quarters of a minute after the brain had been crushed. No blood spouted out, and very little ran from the vessels. A strong ligature was passed round the neck of another rabbit of the same age. It was suddenly tightened and the head cut off. In this instance little spasm took place, and the heart was found beating regularly under the finger, for about three quarters of a minute. At the end of this time the ligature was slackened, and the blood spouted out to the distance of three feet, and continued to spout out with great force till nearly the whole blood was evacuated."

Dr Philip, by the other experiments, shews that the power of the heart may in like manner be destroyed by suddenly crushing a considerable portion of the spinal marrow, although the power of the heart is not at all impaired by removing any part or the whole of it.

These facts, it is evident, afford an easy solution of M. le Gallois' difficulties. He destroyed the spinal marrow by a stilet, of exactly the same dimensions with the cavity of the spine, forced into it; consequently he produced the same effect on the heart as crushing the brain or spinal marrow did in the experiments we are considering. In his experiments with the brain, no effect was produced on the heart, because he did not crush the brain, but removed it, either wholly or by portions. When the spinal marrow was destroyed by portions, the shock was not sufficient to destroy the power of the heart. Thus, in the above experiments, when the spinal marrow was destroyed by a comparatively small wire, little effect was produced on the heart. When it was crushed by a thick wire, or the blow of a hammer, the power of the heart was destroyed. It was the circumstance of M. le Gallois' not being aware that the rapidity with which every portion of the spinal marrow is destroyed influences the result, that occasioned him to receive opposite results from apparently the same experiment. He could not understand why, when he destroyed the cervical part of the spinal marrow by crushing it at once by the stilet, the power of the heart should be destroyed; when, if he destroyed the same or a larger portion by small parts, the power of the heart still continued. The same observation applies to the experiment in which, after the spinal marrow had been divided, crushing either half was found to destroy the power of the heart, although a larger portion of the spinal marrow had been destroyed in other experiments without producing this effect. In these it was destroyed more slowly.

From the whole of his experiments and observations, Dr Philip concludes,

"1. That the muscles of involuntary motion obey the same laws with those of voluntary motion. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, 12, 13, 14, 15, 16, and 17, compared with Exp. 18 and 19. See also observations under Exp. 19.

"2. That the apparent difference in the nature of these muscles arises from their being under the influence of different stimuli. See observations under Exp. 19.

"3. That they are both capable of being stimulated through the nervous system. Exp. 12, 13, 14, 15, 16.

"4. That the power of both is independent of the nervous system. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, 18, 19.

"5. That what is called the nervous system consists of two parts, whose existence is not immediately dependent on each other; the one performing the sensorial functions, the other conveying impressions to and from the sensorium, and without bestowing any power on the muscular system, acting as a stimulus to it. See the observations under Exp. 19; also Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, 18, 19.

"6. That there is, therefore, in the most perfect animals, a combination of three distinct vital powers, not immediately depending on each other; one of the muscular system, one of the nervous system properly so called, and one of the sensorial system. See observations under Exp. 19.

" 7. That the muscular system, though independent of the nervous system, is so influenced by it, that the power of the former may even be destroyed through the nervous system. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 10, 11, compared with Exp. 18, 20, and 23.

" 8. That both the muscular and nervous systems, though independent of the sensorial system, are so influenced by it, that they may even be destroyed through it. Exp. 8, 10, 11, 17, and two last paragraphs under Exp. 19, compared with Exp. 20, 21, and 22.

" 9. That although, in the less perfect animals, we find the muscular life existing alone, and the muscular and nervous existing without the sensorial life, in the more perfect animals they are so connected that none can exist long without the others. See the last paragraph under Exp. 19.

" 10. That nutrition, circulation, and respiration, are the means by which they are so connected."

VII.

Experiments to ascertain the influence of the Spinal Marrow on the action of the Heart in Fishes. By Mr WILLIAM CLIFT. From the Philosophical Transactions of 1815.

THESE experiments relate to the same subject. Mr Clift was led to make them upon the carp, as an animal exceedingly tenacious of life, and having a simple mode of respiration. They furnished him with the following results:

" 1. That the muscles of the body of a carp, four hours after the brain and heart are removed, can be thrown into powerful action.

" 2. That the moment the spinal marrow is destroyed, these muscles lose all power of action.

" 3. That when water is admitted into the pericardium, and the fish allowed to swim about, the action of the heart ceases sooner than when that organ is exposed to air, and the fish kept quiet.

" 4. That whether the heart is exposed or not, its action continues long after the spinal marrow and brain are destroyed, and still longer when the brain is removed without injury to its substance.

" 5. That the action of the heart is accelerated for a few beats, by exposure of that organ; by exposure of the brain; injury to the brain; destruction of the spinal marrow while connected with the brain; by the connection between the brain and spinal marrow being cut off: while removing the whole brain produces no sensible effect upon the heart's action, and destroying the spinal marrow after it is separated from the brain, renders the action of the heart slower for a few beats."

PART III.

MEDICAL INTELLIGENCE.

REPORT of DISEASES treated at the PUBLIC DISPENSARY, near Carey-Street, London, from May 31st to August 31st 1815.

ACUTE DISEASES.

	No. of Cases.		No. of Cases.
Quotidiana	1	Hepatitis	5
Tertiana	1	Enteritis	2
Quartana	1	Dysenteria	13
Febris	54	Cholera	11
—— infantum remittens	9	Peritonitis	3
Otalgia	1	Rheumatismus	1
Hydrocephalus	3	Variola	4
Epistaxis	1	Varicella	1
Cynanche tonsillaris	2	Rubeola	1
Pleuritis	2	Scarlatina	12
Peripneumonia	8	Herpes zoster	1
Catarrhus	58	Erysipelas	1
Pertussis	5		

CHRONIC DISEASES.

	No. of Cases.		No. of Cases.
Vertigo	11	Constipatio	3
Cephalæa	35	Lumbrici	1
Asthma and Dyspnœa	4	Hæmorrhois	2
Catarrhus chronicus	13	Physconia	2
Hæmoptysis	18	Marasmus	7
Phthisis	29	Nephralgia	10
Aneurisma	1	Dysuria	1
Palpitatio	3	Hæmaturia	2
Dyspepsia	36	Hysteralgia	9
Gastrodynia	22	Menorrhagia	8
Pyrosis	1	Amenorrhœa	8
Hepatalgia	1	Leucorrhœa	7
Icterus	1	Asthenia	24
Peritonis chron.	3	Melancholia	2
Colica pictonum	5	Hysteria	11
Enterodynia	32	Epilepsia	2
Diarrhœa	19	Paralysis	4

CHRONIC DISEASES continued.

	No. of cases.		No. of Cas.
Rheumatismus chronicus	38	Lepra	4
Pleurodyne	7	Psoriasis	4
Lumbago	7	Urticaria	3
Hydrops	7	Purpura	2
Hydrothorax	1	Impetigo	1
Anasarca	2	Porrigo	11
Struma	2	Ecthyma	3
Strophulus	1	Scabies	12
Lichen	2	Rupia	1
Prurigo	1	Eczema	1

The summer has been in general warm and dry, and in August the heat in the metropolis was considerable, and the harvest was early and rapidly got in throughout the southern and midland counties.

The fineness of the season might indeed be almost inferred from a perusal of the preceding catalogue of diseases, in consequence of the diminution of the proportion of pulmonary and rheumatic complaints, and the great augmentation of the derangements of the alimentary canal, under the heads of dyspepsia, enterodynia, cholera, &c. as well as of the summer-fevers, headach, vertigo, &c. which are indirectly the result of the same derangements. These maladies, in fact, constitute about one-third of the whole catalogue. The fevers, though unusually numerous, were very seldom severe, or of long duration; some of the patients even walked to the Dispensary, complaining much of headach, with great languor, lassitude, and aching of the back and limbs, and exhibiting a good deal of depression in their countenances, but speedily recruiting after the operation of a purgative, and the use of saline medicines. All the cases of fever were sporadic, or at least were not of a contagious or spreading character, and only two assumed a typhoid appearance, one of which, indeed, was speedily fatal, the skin being covered with petechiæ, and ultimately with large vibices and ecchymoses, which were mistaken by the people for confluent small-pox. Yet in this instance, no other person in the family suffered. Many of the cases of headach and vertigo were obviously but slighter degrees of the febrile affection, and generally yielded to the same remedies, without any topical means of relief being resorted to. In the many cases of bowel-derangement, which were accompanied by considerable abdominal pain, the union of a narcotic with purgatives, especially those of a warmer description, proved very beneficial.

In two cases of inveterate dysentery, which had been long neglected or mismanaged, and to which the ordinary combinations of purgatives and opiates, and other means, failed to afford any material relief, an active treatment, first recommended in this Journal * by Mr Playfair, as practised at Calcutta, and subsequently by Mr English, in London, † was adopted with great and effectual benefit. The relief, indeed, was described in such strong language by both patients, and the request for a repetition, on some threatening of a return of the symptoms afterwards, was so urgent, as to demonstrate the effectual operation of the medicine. The remedy consists in giving a large dose of ipecacuanha combined with a full dose of laudanum. These gentlemen recommend from 30 to 60 grains of ipecacuanha, to be given with the same number of drops of laudanum; a dose which is often vomited at first, but retained on being repeated, and then produces an easy evacuation of the bowels, without tenesmus or blood. In the two cases in question, only a scruple of ipecacuanha was given, with 30 drops of laudanum, and no vomiting was produced, but a speedy relief to the griping and tenesmus, and one or two quiet motions in the day, instead of the numerous irritating discharges, which had previously been the source of continued distress to the patients. This method of administering ipecacuanha in dysenteric affections appears, therefore, to be worthy of attention; and the result of these trials confirms the observation of Mr English, that the remedy is as effectual in the advanced stages of the disease as Mr Playfair had found it in the incipient.

The bowel-complaints of the season, which have assumed a considerable variety of form within the district of the Dispensary, are said to have put on more generally a dysenteric character in the eastern part of the town and suburbs, especially about Hoxton, Bethnal Green, &c. where an epidemic dysentery has been very prevalent, and in a few cases fatal.

London, August 31, 1815. T. B.

On the 1st of August, the Senatus Academicus of the University of Edinburgh conferred the degree of Doctor in Medicine on the following gentlemen, after having gone through the appointed examinations, and publicly defended their respective inaugural dissertations:

	Of Scotland.
Bannatyne Wm. Macleod,	*De Enteritide.*

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* See the No. for January 1813, Art. IV.

† See the No. for October 1814, Art. XII.

Thomas Munro Scott,	*De Apoplexia Phreniticæ.*
James Daun,	*De Phthisi Pulmonali.*
Alexander Tweedie,	*De Cataracta.*
Hugh Smith,	*De Usu externo Aquæ Frigidæ in Febribus.*
John Craig,	*De Phthisi Pulmonali.*
James Copland,	*De Rheumatismo.*
William M. Walker,	*De Mania.*
John Ferguson,	*De Hydrocephalo Acuto.*
William Dalgairns,	*De Chorea Sancti Viti.*
Ebenezer Young,	*De Sanitate et Morbis.*
James Hosack,	*De Dysenteria.*
James Fairbairn,	*De Hydrocele Tunicæ Vaginalis.*
Alexander Ewing,	*De Typho Mitiori.*
John Marshall,	*De Epilepsia.*
John Ross,	*De Tetano Traumatico.*
David Lamb,	*De Phthisi Pulmonali.*
John Lamb,	*De Hernia Inguinali.*
Robert Rutherford,	*De Ascite Abdominali.*
George Thomson,	*De Dysenteria.*
James Brunton,	*De Febre.*
Richard Huie,	*De Plantarum Generatione.*
Thomas Charles,	*De Hepatitide Acuta.*
Andrew Crawford,	*De Typho.*
Mackenzie Bagrie,	*De Apoplexia Sanguinea.*
Robert Hamilton,	*De Asphyxia.*
James Morison,	*De Pneumonia.*
John Logan,	*De Erysipelate.*
Patrick White,	*De Ovario.*
Colin Sievwright,	*De Gastritide.*
William Baillie Ross,	*De Typho.*
Edward Milligan,	*De Hernia præcipue inguinali et crurali.*
Adam Martin,	*De Apoplexia Sanguinea.*
John Belfrage,	*De Viribus Cæli in Morbis inducendis.*
Archibald Arnot,	*De Febre Remittente.*
James Comb,	*De Neuralgia.*
Thomas Shortt,	*De Febre Endemica.*
John Campbell,	*De Angina Pectoris.*
From England.	
Henry Lee,	*De Oculi Morbis.*
William Hall Gilby,	*De Mutationibus quas ea quæ e terra gignuntur aeri inferunt.*
Gavin Ball,	*De Aure Humana et ejus Morbis.*
Joseph Canham,	*De Hydrope*
Edmund Lyon,	*De Asthmate.*
Thomas Seeds,	*De Sanguine Misso.*
Peter Jackson Skerratt,	*De Hydrocele.*
John Eccles,	*De Dyspepsia.*
William Berthon Scott,	*De Lauro Ceraso.*
John Edward Freake,	*De Ventriculo.*
Thomas Harrison Burder,	*De Morbis Syphiloïdeis.*
Thomas Addison,	*De Syphilide et Hydrargyro.*
John Somers Down,	*De Febre Inflammatoria Biliosa.*
Charles Edward Pratt,	*De Ophthalmia Purulenta.*
John William Polydori,	*De Oneirodynia.*

	From IRELAND.
William Moore,	*De Bile et ejus Calculis.*
William Trail,	*De Neuralgia.*
Joseph Ewing Miller,	*De Gastritide.*
William Catherwood,	*De Nostalgia.*
Zachariah Maxwell,	*De Chancre.*
Robert Shaw,	*De Hepatitide.*
Thomas Verdon,	*De Pneumonia.*
Robert Henderson,	*De Pneumonia.*
Dillon Kelly,	*De Peritonitide Puerperarum.*
Rogers Cotter,	*De Tympanita.*
James Gordon,	*De Tumore Albida.*
Thomas Breen,	*De Structura Coxæ et Morbis a vi externa orientibus.*
Robert Cardiff,	*De Febre Puerpera.*
Robert Murray,	*De Circulatione Sanguinis in Fœtu.*
Thomas Burke,	*De Dysenteria.*
George W. Patton,	*De Febre Puerperarum.*
Edward Johnson,	*De Febre Intermittente.*
John Prendergast,	*De Febre continua Contagiosa.*
James Butler,	*De Hepatitide.*
Thomas Moore,	*De Dysenteria.*
Walter M'Donnell,	*De Ophthalmia.*
James Devlin,	*De Typho.*
William Isaac Morgan,	*De usu Electricitatis in re Medica.*
	From MADEIRA.
Laurence Joseph Moniz,	*De Ictero.*
	From FAYAL.
John da Silveira Caldeira,	*De Febribus Intermittentibus.*
	From the WEST INDIES.
Thomas Anderson,	*De Morbis Cæli mutatione levandis.*
	From JAMAICA.
George William Towton,	*De Tetano.*
	From ST CROIX.
Abraham Armstrong,	*De Tetano.*
	From TRINIDAD.
John Baptist Philip,	*De Affectibus Hystericis.*
	BRAZILS.
Joachim Bernard Pereira,	*De Erysipelate et Hydrocele.*

Some account of Margaret Lyal, or the Sleeping Girl of Montrose, whose case appears to have been a remarkable one of Carus. Communicated from observation, By Mr JAMES ARROT, Fellow of the Royal College of Surgeons, Edinburgh.

MARGARET LYAL, ætat 19, who had hitherto enjoyed good health, left the service of Mr Arkley of Dunninald, on the 26th of May, and on Saturday the 27th went home to Mrs Foote of Logie-Park, in the vicinity of Montrose. On the 31st, the Wednesday following, she was attacked with a nervous fever, that confined her seventeen days, the first ten of which was at Mrs Foote's, and the remainder at her father's, a shoemaker, residing near Dunninald House. On the 24th of June, being recovered from her fever, she returned to her mistress, now living at the Budden for sea-bathing, about a mile from her father's. On Wednesday the 28th, four days after her return, she awoke about two o'clock in the morning, bleeding profusely from the nose, which continued for several hours. The loss of blood must have been considerable, but the exact quantity cannot well be ascertained, from her having got out of bed, and allowed it to flow into the ash-hole; immediately after this, she fell into a comatose state, during which, at the expiration of two days, she was carried in a cart to her father's house, and remained fifteen hours longer in the same state. At the expiration of this period she awoke, and took some food, complaining only of weakness. About fifteen hours from this time, being the first of July, she again became comatose, and continued in this state seven days and seven hours, without taking any kind of nourishment, and without the evacuation of either feces or urine; after which some food was given, by a person putting the patient's hand to her mouth with a spoon, and pouring it in, by which she was enabled to swallow, although still comatose, but the hand remained in the same situation, till put back by the assistant. In this state she continued to the 7th of August, thirty days more, receiving nourishment in the same manner occasionally, and passing her feces and urine involuntarily. On the 7th, some signs of hearing were evident, and about half past ten o'clock at night, she was lifted from her bed, and in the course of a few minutes after, all her faculties returned. She was, however, in an extreme state of debility, and very much emaciated. In the course of the first sixty-three hours a pound of blood was taken from her arm, and about three weeks after her head was shaved and blistered. Sinapisms were occasionally applied to the feet, and her bowels were kept open with laxatives and enemas, but without producing any favourable change. She is now rapidly regaining her health and strength; her appetite is good, and she is able to take moderate exercise; but has not the slightest recollection of any thing that happened to her, during the long comatose state—not even dreaming; and could hardly believe the state she had been in had been of more than two or three days duration.

Edinburgh, 11*th Sept*. 1815.

Dr W. Philip of Worcester has been farther prosecuting the inquiry, of the published part of which we give an account in the present number of our Journal; and has presented to the Royal Society of London his additional experiments and observations. In these we are informed he had two objects in view; the one to ascertain whether the blood-vessels, like the heart, possess a power independent of the nervous system, and whether, like it, they are capable of being directly influenced through that system; the other, to ascertain whether the laws which regulate the effects of agents, applied to the nervous system, on the muscles of voluntary and those of involuntary motion, are the same.

His experiments shew, that the blood-vessels are capable of performing their functions after the nervous system is wholly removed; but that while that system, and the connection of nerves are entire, they are, like the heart, directly subjected to the effect of agents applied to the nervous system. He shews by many experiments, that the laws which regulate the effects of agents, applied to the nervous system, on the muscles of voluntary and those of involuntary motion, are not the same; but differ in the following respects: No agents applied to the nervous system affect the muscles of voluntary motion, except those which make their impression on the nerves, or on those parts of the brain and spinal marrow, near which the nerves originate. The muscles of involuntary motion are equally affected by agents applied to every part of the brain and spinal marrow. No agent affects the former muscles but one of great power. No agent affects the latter, that does not act on a considerable portion of the brain or spinal marrow. By all agents capable of exciting the muscles of voluntary motion, we may excite irregular action in them. We can excite no irregular action in the heart and blood-vessels by any means, but such as suddenly destroy a large portion of the nervous system. We only increase or diminish the frequency and force of their action. Stimuli applied to the nervous system are capable of increasing or even renewing the action of the heart and blood-vessels, long after the nervous system has lost all power over the muscles of voluntary motion. These and other differences he finds all referable to the two first mentioned.

One, among numerous inferences which he draws from his experiments, is the use of the ganglia, which he thus explains: We have just seen that the muscles of involuntary motion obey stimuli applied to every part of the brain and spinal marrow. They do not receive nerves from every part of these organs; but they receive nerves from a chain of ganglia, which receive nerves from every part of the brain and spinal-marrow. Thus, it is proved, that the nerves issuing from ganglia, convey the influence of all the nerves which terminate in them; and consequently, that the ganglia and the nervous matter which connects them, may be regarded as a reservoir of nervous influence, from which all those parts, which are subjected to the whole nervous system, are supplied; those which are subjected to particular parts of this system, being supplied directly by nerves from those parts. If such are the facts, it is evident that, until it was discovered that the heart is influenced by every part of the nervous system, it was impossible that we should arrive at a knowledge of the use of the ganglia.

Speedily will be published, The Principles of Diagnosis. By MARSHALL HALL, M. D. &c. This work will embrace, 1. A View of the Countenance and Attitude of Patients, as characteristic of different Diseases. 2. The Symptoms of Diseases, considered in their Modifications, and in relation to Individual Affections. 3. A Diagnostic Arrangement of Diseases; and, lastly, their Diagnosis.

Dr BALFOUR is preparing, and will speedily publish, Observations, with Cases, illustrative of a new, simple, and expeditious Mode of curing Rheumatisms and Sprains, without at all weakening the System. In this Work, some new and interesting Facts relative to Inflammation and Palsy, will also be noticed.

In the Press, and speedily will be published, by E. Cox and Son, St Thomas's Street, Borough, a Second Edition of Mr ASTLEY COOPER's large work, on the Anatomy and Surgical Treatment of Hernia.

Also, in a few weeks will be published, a Second Edition of Dr WILLIAM HUNTER's Large Plates of the Anatomy of the Human Gravid Uterus.

Dr JOHN BOOTH of Birmingham is preparing for the Press, Observations on the Diseases incident to Persons occupied in Trades and Manual Arts.

Dr SCUDAMORE of Holles Street, Cavendish Square, London, is preparing for the Press, and will publish, in the course of the ensuing Winter, A Treatise on the Nature and Cure of Gout, comprehending a general View of a Morbid State of the Digestive Organs, and of Regimen; with some Observations on Rheumatism.

Mr POWER is preparing for the press a second edition, with additions, of his work on the Cause and Cure of the Egyptian Ophthalmia, to which will be added Observations on the Cause, Nature, and Cure of Acute and Chronic Ophthalmia, as it exists at present in the British army at home.

OBSTETRICAL THEATRE, 68, Berner's Street.

The winter course of Lectures on the Science and Practice of Midwifery, including the Diseases of Women and Infants, by Dr CLOUGH, Physician Accoucheur to the St Mary-la-bonne General Dispensary, &c. &c. will commence at his Theatre, on Monday morning, the 2d of October, at half-past ten; and his Evening course at seven.

THEATRE OF ANATOMY, MEDICINE, &c. Blenheim Street, Great Marlborough Street.

The Autumnal Course of Lectures at this School will begin on Monday October 2, 1815.

Anatomy, Physiology, and Surgery, by Mr BROOKES, daily at Two. Dissections as usual.

Chemistry, Materia Medica, &c. daily at Eight in the Morning; Theory and Practice of Physic at Nine; with Examinations, by Dr AGER.

Three Courses are given every Year, each occupying nearly Four Months. Farther particulars may be known from Mr BROOKES at the Theatre, or from Dr AGER, 69, Margaret Street, Cavendish Square.

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